

EXPERIMENTS
AND
OBSERVATIONS
ON DIFFERENT KINDS OF
AIR,
AND OTHER BRANCHES OF
NATURAL PHILOSOPHY,
CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;
Being the former Six Volumes abridged and methodized, with many
Additions.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. ITAL. HARLEM. AUREL.
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V O L. II.

Fert animus causas tantarum expromere rerum,
Immensumque aperitur opus.

LUCAN.

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B O O K III.
OBSERVATIONS RELATING TO NITROUS
AIR.

P A R T III.
EXPERIMENTS TENDING TO ASCERTAIN THE CON-
STITUENT PRINCIPLES OF NITROUS AIR.

AS the circumstances in which any kind of air is decomposed tend to discover its composition, I shall under this head recite all the observations I have made of this kind.

SECTION I.
Of the Diminution of nitrous Air by standing in Water.

BEING desirous of ascertaining whether nitrous air tended, in any degree, to a spontaneous decomposition by long standing only, without being in contact with any substance that could be supposed to have an affinity with any constituent

part of it, I filled a large phial with this air, and corked it very tight, keeping it for the most part with its mouth immerfed in a vessel of water, but afterwards neglected it, and suffered the water to evaporate. However, examining this air after about *two years*, I found that it had the same power of diminishing common air that fresh-made nitrous air has. It was also not more easily decomposed by agitation in water; and I would observe in this place, that I have found great differences in the readiness with which nitrous air is diminished, and reduced to the state of phlogisticated air, by agitation in water, or by simply standing exposed to a considerable surface of water; a difference depending on the *water*, and the impregnations of it; but I have not given so much attention to these circumstances as to have discovered the cause of them.

I have observed that if nitrous air be agitated in an open trough of water presently after it is made, or indeed after it has been kept some weeks, it will be reduced to a very small quantity, perhaps one twentieth of its original bulk; and it will then be wholesome air. But I find that if it be kept a very *long time*, its constituent principles, as we may say, acquire a much firmer consistence, and that then a remarkably greater proportion of it becomes first phlogisticated air, and then, by agitation in water, wholesome air.

I once

I once observed the changes produced in two quarts of nitrous air, one from *iron*, and the other from *copper*, made in November, 1773. On removing from Wiltshire to Birmingham, I thought proper to put an end to this process, when I found no farther change in the bulk of these two quantities of air; that which had been produced from iron still occupying two thirds of its original dimensions, and that from copper one half. I agitated in water a portion of each of these quantities of air, without producing any change in their bulk; but they were both considerably improved by it, so that when mixed with equal quantities of fresh nitrous air, the measures of the test were 1.75.

This I consider as a pretty remarkable observation, as it exhibits a change in the constitution of a body depending upon *time* only, and which it is not yet in our power to produce by any other agent or instrument.

Both nitrous and inflammable air contain phlogiston, and, as will be seen in its proper place, they probably contain nearly equal quantities in equal bulks; but as their properties are remarkably different, their constitution must be different also; the phlogiston which enters into the composition of them both being combined in them in a very different manner. In some cases nitrous air parts with its phlogiston more readily than in-

flammable air, but in other respects inflammable air is the more easily decomposed of the two. The phlogiston of nitrous air immediately quits it on the contact of common air, when it is even quite cold, whereas the phlogiston of inflammable air will not leave it to join the common air except when it is very hot; but it will be seen that inflammable air parts with its phlogiston to the glass of lead in the composition of flint glass in circumstances in which nitrous air undergoes no change whatever. I kept a quantity of nitrous air in a tube of flint glass, hermetically sealed, buried in hot sand, but not sufficient to melt the glass, twenty days without any sensible change in the bulk or quality of the air. In the upper part of one tube filled in this manner there was something like small crystallizations, but they might possibly come from a small quantity of the quicksilver accidentally left in the tube. But whether nitrous air will be decomposed by quicksilver in this state of heat and confinement I did not try. Indeed I did not examine whether what I saw were properly crystallizations, or not.

I kept both nitrous air and inflammable air very hot in contact with quicksilver with liberty to expand, and did not find that either of them underwent any change. A quantity of nitrous air I exposed several hours for three days to a degree of
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of heat which kept the quicksilver in a state of vapour, the first and second days to the same quicksilver, and the third day to fresh quicksilver; but the dimensions of the air, and its property of affecting common air, continued the same. The process is described in the Introduction.

The addition of steam of water to the nitrous air in this state of heat and expansion made no difference in the result of the experiment, though they continued together upon quicksilver more than two hours. The small alteration that I found in the nitrous air might be ascribed to its having been transferred from the trough of water to the basin of quicksilver in a bladder. I varied the experiment by confining the nitrous air in a glass jar inverted in a pan of water, which I made to boil, in order that the hot steam might pervade the whole mass of the air, which it effectually did, as it appeared by its having expelled a very great part of it. After the process, which continued about an hour, the nitrous air had lost nothing of its power of diminishing common air. On the contrary, it seemed, to be rather improved than to have had its virtue impaired,

SECTION II.

*Of the Diminution of nitrous Air by Iron Filings
and Sulphur, and by a Solution of green Vitriol.*

THE diminution of common air by a mixture of nitrous air, is not so extraordinary as the diminution which nitrous air itself is subject to from a mixture of iron filings and sulphur, made into a paste with water. This mixture, as I have already observed, diminishes common air between one fifth and one fourth, but has no such effect upon any kind of air that has been diminished, and rendered noxious, by any other process; but when it is put to a quantity of nitrous air, it diminishes it so much, that no more than one fourth of the original quantity will be left.

The effect of this process is generally perceived in five or six hours, about which time the visible effervescence of the mixture begins; and in a very short time it advances so rapidly, that in about an hour almost the whole effect will have taken place. If it be suffered to stand a day or two longer, the air will still be diminished farther, but only a very little farther, in proportion to the first diminution.

The

The glass jar, in which the air and this mixture have been confined, has generally been so much heated in this process, that I have not been able to touch it.

Nitrous air thus diminished has not so strong a smell as nitrous air itself, but smells just like common air in which the same mixture has stood; and it is not capable of being diminished any farther, by a fresh mixture of iron and brimstone.

Common air saturated with nitrous air is also no farther diminished by this mixture of iron filings and sulphur, though the mixture ferments with great heat, and swells very much in it.

To the experiments upon iron filings and sulphur in nitrous air, I must add, that when a pot full of this mixture had absorbed as much as it could of a jar of nitrous air (which is about three fourths of the whole) I put fresh nitrous air to it, and it continued to absorb, till three or four jars full of it disappeared; but the absorption was exceedingly slow at the last. Also when I drew this pot through the water, and admitted fresh nitrous air to it, it absorbed another jar full, and then ceased. But when I scraped off the outer surface of this mixture, which had been so long exposed to the nitrous air, the remainder absorbed more of the air.

When I took the top of the mixture which I had scraped off, and threw upon it the focus of a burning glass, the air in which it was confined was diminished, and became quite noxious; yet when I endeavoured to get air from this matter in a jar full of quicksilver, I was able to procure little or nothing.

Nitrous air is as much diminished both by iron filings, and also by liver of sulphur, when confined in quicksilver, as when it is exposed to water.

There is a remarkably quick absorption of nitrous air by a solution of green vitriol, the particulars of which are as follows.

Having dissolved a quantity of green vitriol, and put it into a phial, with its mouth inverted in a basin of the same, and having admitted a quantity of nitrous air to it, I began to agitate the solution, in the same manner as in the process for impregnating water with fixed air; when I observed that the nitrous air, in these circumstances, was absorbed much more readily than fixed air is by water. I even made a quantity of this solution absorb more than ten times its bulk of nitrous air, without any very sensible approach to saturation. This solution became black by this process; but when a small part of it was viewed by the light

light of a candle, placed beyond it, it looked red. The taste of the solution was acid, owing, no doubt, to the mixture of nitrous acid, which it had acquired, in consequence of the decomposition of the nitrous air.

When this impregnated solution was exposed to the open air, large green crystals were formed at the bottom of the vessel, and all the black colour intirely disappeared. But when these crystals were formed at the bottom of a very tall vessel, they were much blacker, and did not even become green on being exposed afterwards to the open air, any more than those which I exposed to nitrous air itself on quicksilver.

The changes of colour, and all the phenomena of the crystals, were evidently owing to the spirit of nitre contained in the nitrous air, and set at liberty in its decomposition. For a few drops of the acid itself produced the same effects, in all respects, on this solution.

Conceiving that the principal of these phenomena must have arisen from the affinity between nitrous acid and iron, I agitated nitrous air in a natural chalybeate water, when it presently became of a brownish colour, which seemed to be a confirmation of my supposition.

I also

I also made another experiment in which the nitrous acid might show its affinity to iron in a manner somewhat similar to this. I first saturated a quantity of water with fixed air, then with iron, and afterwards impregnated it with nitrous air. The result of this experiment was, that the solution assumed a colour between green and yellow; but it did not absorb much more nitrous air than water unimpregnated with fixed air, or with iron, would have done.

The nitrous air which I had hitherto made use of in these experiments was made from copper, but when I used that which was made from iron, which is an ingredient in green vitriol, the effect was not at all different. The solution of the vitriol absorbed this nitrous air with the same rapidity as it did that which was made from copper, and the subsequent phenomena were also, in all respects, the same.

I then agitated nitrous air in solutions of blue and white vitriol, the former of which is known to be composed of copper, and the latter of zinc. The result was, that the colour of both these solutions became presently very dark, the former changing into a deep green, and the latter into a kind of brown. Not more than between one half and one third of the air (which was about one fourth

of the contents of the phial I made use of) was absorbed in either of these cases, which is very far short of the effect of the solution of green vitriol on the same kind of air.

It made no difference whether the nitrous air was procured from iron or from copper, in any of these experiments. For the solution of green vitriol, as I have observed, decomposed nitrous air made from iron just as readily as that which was made from copper; and, on the other hand, the solutions of blue and white vitriol were affected in the very same manner by nitrous air made from copper, as by that from iron.

The solution of white vitriol deposited a white and flocculent matter, and then was transparent like water; but, being impregnated with nitrous air, it presently became of as dark a colour as when it had been impregnated before that deposit was made.

Spirit of nitre dropped into the solution of blue or white vitriol made little or no change in their colour.

All the solutions of vitriol which had their colour changed by the impregnation of nitrous air recovered it again by exposure to the common air. This was evidently effected by the escape of that phlogiston, which had contributed to the deepness of
of

of their colour. To ascertain this, I filled a phial about three fourths full of the solution of green vitriol, made black by the decomposition of nitrous air, and after about a week, examining the air which had been confined with it, I found it to be so much phlogisticated, that one measure of it and one of nitrous air occupied the space of 1.92 measures.

Upon the whole, it seems that the greater effect of the solution of green vitriol in decomposing nitrous air must be owing to the stronger affinity between the spirit of nitre and iron, than between the same acid and copper or zinc.

They seem to show, however, that there is little, if any martial earth in nitrous air, at least, that such earth existing in nitrous air is not combined with phlogiston, or in a metallic state; since this air is decomposed by the nitrous acid in it quitting the phlogiston with which it was already combined, in order to unite itself to the iron in the solution, at the same time that the phlogiston which entered into the nitrous air contributes to blacken the solution. It will, perhaps, however be thought extraordinary, that the nitrous acid should have a stronger affinity with iron than the vitriolic, which, on this hypothesis, it must, in this particular case, have.

This effect of the solution of vitriol on nitrous air helps to explain a phenomenon, which I had often

often observed without understanding it. When the water in my trough had got impregnated with various metallic substances, that which was contiguous to the nitrous air, in jars standing in it, would be of a darker colour than the rest of the water. This must have been in consequence of the affinity between the phlogiston in the nitrous air and the metallic matter dissolved in the water, by means of some acid that happened to be mixed with it. At one time, when the water in my trough was particularly foul, and seemed disposed to make a deposit, I impregnated part of it with nitrous air, and the water, by this means, presently became of a darker colour than before.

To determine whether the phenomena attending the impregnation of the solution of green vitriol with nitrous air depended, in any measure, upon the seeming *astringency* of that solution, and of chalybeate waters, I impregnated a quantity of *green tea*, which is also said to be astringent, with nitrous air, but no sensible change of colour was produced in it.

In my first publications I mentioned a variety of circumstances in which nitrous air is remarkably diminished, in several of which it passes through a state in which a candle burns in it quite naturally, and sometimes with a much enlarged flame,

flame, and at last becomes mere phlogisticated air. In all these processes I took it for granted (not having examined the air except when it was completely, or, at least, very nearly reduced to one of the two states above-mentioned) that the approximation to its final state of phlogisticated air was equable, so that as soon as it began to be diminished, it also began to lose its power of affecting common air. I find, however, that, with respect to several of the causes of diminution, and perhaps all of them, the air passes very suddenly from the state in which it is perfect nitrous air, to the state above-mentioned; but that the term at which this change takes place is various, as sometimes two thirds, and sometimes fourteen fifteenths of any quantity on which the experiment is made, will have disappeared before any sensible change can be observed in the remainder. I have even sometimes been inclined to think that its power of affecting common air has been rather increased than diminished at the beginning of these processes.

I imagine, therefore, that, as soon as either the nitrous principle, or the phlogiston which enters into the composition of nitrous air, is seized upon by any substance which has a stronger affinity with either of them than they have with each other, so much of the other principle as was combined with it

it is precipitated, so that the air which remains is not at all altered from what it was, at least for a considerable time. It will appear, however, that the slower the process is, the greater quantity of nitrous air will be preserved in the state of phlogisticated air, and the quicker the process, the farther it will proceed before this change takes place.

I entertained the first suspicion of my having been mistaken in my former opinion when I was examining some nitrous air in which I had confined a fowl, in order to preserve it as long as possible from putrefaction. For though this air was greatly diminished in quantity, it affected common air quite as much as the best nitrous air I had ever tried.

Being desirous of ascertaining this fact with absolute certainty, with respect to some one cause of the diminution of nitrous air, I placed a pot of *iron filings and brimstone* in a jar of nitrous air, and let it remain there a whole day, keeping it generally warm, near the fire, the ingredients not being good of their kind, and not disposed to ferment. As the diminution proceeded, I kept taking from it small portions of the air, by introducing into it a small jar full of water; which, being emptied within the jar, I withdrew, filled with the air from within it. Doing this occasionally, I observed no change

change of the quality of the air when it was reduced to one third of its original bulk, for it retained its full power of diminishing common air.

The next day I found it diminished to one fourth of the whole, and then a candle burned in it in a manner not to be distinguished from the burning of a candle in common air. But it was not common air; for it was not at all diminished by fresh nitrous air, and it affected common air so little, that one measure of it and one of common air occupied the space of 1.85 measures. It had not acquired the peculiar property of fixed air, for it did not make lime water in the least degree turbid, and it bore considerable agitation in water without being much diminished.

The diminution of nitrous air by means of *spirit of nitre* is effected in the same manner; and as this diminution was made more quickly, on this account, perhaps, it proceeded much farther before I could perceive any change in it. In one experiment of this kind, I thought the change took place when the air was diminished to one twelfth of the whole, but in another case there was no change till it was reduced to between one twelfth and an eighteenth part, when it was completely phlogisticated. In this mode of diminution I was not able to find it in that state in which a candle could burn

in it. At another time when exactly one fifteenth of the whole remained, it affected common air manifestly less than fresh nitrous air; but here again, when only one eighteenth remained it had lost all its peculiar property.

The diminution of nitrous air by the *solution of green vitriol* is effected according to the same rule. I decomposed nitrous air by exposing it to be absorbed by the solution of green vitriol till about one fourth of the original quantity remained, but it affected common air as much as it had done before any part of it was absorbed.

Such, also, is the manner in which nitrous air is diminished in a *bladder*. Nitrous air reduced in this manner from ten ounce measures to two and a half, was so much altered, that one measure of it and one of common air occupied the space of 1.75 measures. It then extinguished a candle without any appearance of a blue flame. When a little more of it was absorbed by the same process, I found the remainder all phlogisticated air, not affecting common air in the least. Till the nitrous air was reduced, in this manner, to very near one fourth, it continued unchanged.

If, however, common air be mixed with nitrous air, by which means it becomes in part phlogisticated air, the spirit of nitre will absorb the superfluous nitrous air only, and consequently leave the

remainder more diluted with phlogificated air. I put a measure and a half of common air to two measures of nitrous air, so that one measure of this mixture and one of common air occupied the space of 1.36 measures. I then put some spirit of nitre to the mixture, and when it had absorbed one third of it, one measure of it and one of common air occupied the space of 1.8 measures.

I also introduced a piece of hot *charcoal* into a phial of nitrous air by which means one half of it was absorbed, and found that the remainder had not lost its power of diminishing common air in any sensible degree. The absorption of all kinds of air by *charcoal* is a very capital discovery of the Abbé Fontana, which he has been so obliging as to give me leave to mention.

When nitrous air has been kept a long time in water, it is known to be diminished, and in this case I suspect that it loses its virtue gradually, being impaired from the first. I have found, however, that by long keeping in perfectly stagnant water, the surface of which was exposed to the atmosphere, and without any change, except to supply the waste by evaporation, it came to the state of phlogificated air; but by what steps in the process I omitted to observe, having taken it for granted that this was always equable.

On the 11th of November 1773, I filled two quart bottles with fresh made nitrous air, one from iron, and the other from copper, and then set them aside, with their necks immersed in jars of water, and never agitated the air or the water in contact with it, only supplying the jars with fresh water as I perceived it was wanted. On the 29th of September 1778, I examined the state of these bottles of air, and found as follows. Of that which had been made from iron about one half was absorbed, and of that from the copper about a third; but both of them were equally and perfectly phlogisticated, making no effervescence with common air, and extinguishing a candle. That which had been made from copper did not make lime water turbid, and the same, I doubt not, would have been the case with the other, if it had been tried. I did this from a suspicion, that, since fixed air may be composed from the nitrous acid, nitrous air, in some of its changes, might, in part, assume that form. I had not given much attention to these bottles of air, but I do not think they had been at all diminished the last year, or the last year and a half.

Pyrophorus also decomposes nitrous air, and presently reduces it to the state of phlogisticated air. Having put a quantity of it into a glass jar standing inverted in quicksilver, I introduced some nitrous air to it, when the *pyrophorus* became instantly red

hot. What remained of the nitrous air had no effect on common air, and extinguished a candle. All this change was effected at once. For though the nitrous air continued in the jar a day and two nights after it had been admitted to the pyrophorus, there was no farther change in its dimensions.

Since pyrophorus fires pretty much alike in nitrous and in dephlogisticated air, these two very different kinds of air must contain some common element; and this may be either *water*, or the *acidifying principle*. It is most probable that nitrous air contains both, though in a very different state of combination, and in different proportions than those in which they exist in dephlogisticated air.

The *willow plant*, as I shall observe, absorbs nitrous air as well as every other kind of air. What were the immediate states of it I did not note, but when the air was reduced to one tenth of its bulk, I found it to be mere phlogisticated air.

Nitrous air, as I have observed, is presently decomposed by a solution of green vitriol in water, which, in consequence of it, becomes of a very dark colour; but becomes green again on being exposed to the open air. In this and many other properties, the effect is the same as that of mixing a small quantity of spirit of nitre with that solution. Having impregnated a quantity of the solution in this manner, I heated it in a glass phial, and found that

that it gave out half its bulk of pure nitrous air, but when it had lost its dark colour, it gave no air at all by heat.

I have afterwards observed this change of colour to be a criterion of the presence of a very small quantity of the martial salts in water. When a very little of it has been accidentally formed, in the course of my experiments, and mixed with the water in my trough, I have never failed to discover it by the dark colour of the water in those jars which contained nitrous air. This change of colour must, as I observed before, have been produced by the phlogiston of the nitrous air, the nitrous *acid* having no such effect. This was also the case with a solution of copper in nitrous acid; but the change was not from blue to a darker colour, but into *green*.

I filled a jar, about an inch in diameter, and twelve inches long, with that solution of copper in spirit of nitre which remains after making nitrous air, and which is of a beautiful blue colour. Then inverting it in a basin of the same, I introduced to it a quantity of nitrous air. After some time I observed that the air was considerably diminished, and that all the surface of the liquid in contact with the air, to the depth of about a quarter of an inch, was of a beautiful green colour. This air kept diminishing some months, and the green colour of the solution extended two or three inches within the li-
quid

quid. At last there remained only two sevenths of the original quantity of air; and, examining it in that reduced state, I found it to be mere phlogisticated air. Had it been examined in the intermediate state, it would, I doubt not, have been found to be of that kind of air in which a candle will burn. The experiment was begun on the 4th of October 1779, and the air was not examined till the 20th of July 1780.

SECTION III.

Of the Diminution of nitrous Air by the electric Spark.

THE electric spark taken in nitrous air, diminishes it to one fourth of its original quantity, which is about the quantity of its diminution by iron filings and sulphur, and also by liver of sulphur without heat. The air is also brought by electricity to the same state as it is by iron filings and sulphur, not diminishing common air. If the electric

electric spark be taken in it when it is confined by water tinged with archil, it is presently changed from blue to red, and that to a very great degree.

This experiment I have frequently repeated, in order to ascertain more particularly the quantity and quality of the residuum. In one experiment I reduced half an ounce measure of nitrous air, in less than half an hour, to one quarter of its bulk; but then one fourth of it was still nitrous, and the rest phlogisticated air. Receiving it in lime water, there seemed to be a slight precipitation; but this circumstance will perhaps be explained in the following experiment.

I took the electric spark in a quantity of nitrous air till there remained no more than one third of it, and this was completely phlogisticated, not affecting common air at all, and extinguishing a candle. A white matter was formed with the mercury over which the spark was taken, which made the water admitted to it extremely turbid. At that time I supposed that this white substance was mercurial nitre, consisting of nitrous acid from the decomposed nitrous air and mercury. I shall now examine it more particularly. Why there should be more than one third of this quantity of nitrous air left pure phlogisticated air, and not one fourth, as in the former experiment, I cannot tell.

In another process I took the electric spark in a quantity of nitrous air, till it could be no more diminished; when it was in proportion to its former bulk, as $10\frac{1}{2}$ to 24. Letting it stand all night upon the mercury, it was increased to the proportion of $11\frac{1}{4}$ to 24, seemingly by the acid uniting to the mercury, and generating more nitrous air, since it had that smell. No water appeared after the process, and the water admitted to it acquired no acid taste, but an astringent one, like water impregnated with nitrous air. There was a white powder formed, as in the former experiments.

Thinking to make water imbibe the acid from the nitrous air, decomposed in this process, I took the electric spark in it with a small quantity of water over the mercury. But even this water did not acquire any acid taste, but only an astringent one.

It remains to be ascertained on what principles the nitrous air is decomposed in this process. That it is not by means of the *heat* communicated to it, may, I think, be inferred from an experiment, in which I made a quantity of nitrous air pass through a red hot tobacco pipe, without producing any change in it. Neither does it seem to be the *light* of the electric spark, since I have heated pieces of crucibles in nitrous air by the sun beams, without making any change in it.

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Upon the whole, it may be most probable that this change depends upon *phlogiston*, as it is similar to that which is made in nitrous air by the contact of iron, and especially by heating iron in this air. There is this difference, however, in the two processes; that in that with electricity the residuum is wholly phlogisticated air, and the intermediate state of dephlogisticated nitrous air is not found. At least I have not observed it.

I afterwards repeated with particular care this experiment of decomposing nitrous air by the electric spark; from which it appears that, in this process, though not in that of heating iron in it by the sun beams, some *acid* is really deposited from the air. But it is so little, that it may perhaps be supposed to have been *contained* in it, as an extraneous substance, not separable from it by standing in water. It is also to be observed that, in this process, I was never able to produce any dephlogisticated nitrous air. Whenever the experiment was suspended, the air was always found to be either nitrous or phlogisticated.

In one experiment of this kind, to which I gave particular attention, an ounce measure and a half of nitrous air was reduced to 0.4 of a measure by electric explosions, taken in a vessel of mercury, with a little pure water on the surface of it; an iron wire introduced through the mercury, and into the air, being used for the purpose of taking the explosion
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at a proper distance. After the greatest possible diminution of the air I could perceive no change of colour in the water, but after a few hours standing it had a purplish colour, and after a day and night, it was become of a deep red, and very turbid, evidently from the solution of the iron. The water was acid to the taste, but in the slightest degree imaginable; and I could perceive by the smell of it, that there was a little nitrous air still contained in it, though I could not perceive any redness on exposing the vessel to the open air. A lighted candle being dipped into it, was immediately extinguished.

There was one circumstance which I cannot easily account for in this process. After the diminution of the air would proceed no farther, I observed, as before, that, after some time, there was an increase of the quantity of it, viz. about one tenth of an ounce measure; but after this the air was gradually contracted to its former dimensions, without any farther use of electricity.

I repeated this experiment with the same quantity of nitrous air, and it was diminished in about the same proportion as in the preceding. But examining the water before it had been discoloured by the solution of the iron, I could only perceive some astringency in the taste of it, though it reddened a considerable quantity of water tinged blue with the juice of turnsole. There was also still something
nitrous

nitrous in the residuum of this air. For being mixed in equal quantities with common air, the standard was about 1.8. It contained no dephlogisticated nitrous air, as it was not sensibly diminished by agitation in water.

Lastly, I nearly filled a phial, containing about six ounce measures of nitrous air, confined by quicksilver; and taking the electric spark within it, it was in about an hour diminished about one half, but after that very little. The quicksilver was much corroded, and a candle went out in the remainder of the air. The diminution of common air by the electric spark requires a good deal of time, but this process goes on very rapidly with nitrous air. I repeated the experiment in a tube a quarter of an inch in diameter, receiving the spark upon water tinged blue with the juice of turnsole; and the diminution was so quick, that the motion of the water up the tube was constantly sensible to the eye. The water was deeply and permanently red.

As the electric spark cannot be supposed to communicate so much phlogiston, as the heating of iron by the sun beams, we may be better able by this process to ascertain the quantity of phlogiston contained in phlogisticated air; since the residuum probably contains all the phlogiston belonging to the nitrous air from which it is made, and if we may
calculate

calculate from the numbers in the former of these experiments, phlogisticated air must contain about four times as much phlogiston as an equal bulk of nitrous air; or, as appears by my former experiments, almost four times as much as inflammable air made from iron by oil of vitriol, or steam, of which one half is probably phlogiston, and the other half water.

SECTION IV.

Of the nitrous Acid produced from nitrous Air, by a Decomposition with common, or dephlogisticated Air.

IN order to complete the analysis of nitrous air, the *nitrous acid* which it is capable of forming by means of pure air, and which is a principal ingredient in its composition, ought to be more especially attended to; and it is not difficult, by means either of common or dephlogisticated air, so to decompose it, as to collect almost all the acid that it can assist in forming. And could we apply any certain *measure* to ascertain the strength of this acid,

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we might measure it in this as well as in any other case.

It is well known that when nitrous air is mixed with as much common air as it can completely phlogistificate, it is itself decomposed, and especially that the acid which it assists in forming, immediately appears in the form of a red vapour, which is absorbed by water, if the decomposition be made over water, or imbibed by alkaline or metallic substances, if any such be at hand to unite with it. Now by providing a given quantity of water to imbibe this acid, formed by a known quantity of nitrous air, it will be easy to ascertain the quantity of nitrous acid transferred to the water, by finding how much nitrous air that acidulated water will yield, by the solution of copper, or any other metal. In this manner, at least, the strength of the acid may be compared with the strength of any other nitrous acid in a fluid form.

The apparatus for this experiment is very simple, and very easily applied. I first provide a phial of a convenient size and form for the impregnation of the water, which ought to be pretty large, in order to decompose a considerable quantity of nitrous air at one process; and having fitted a cork to it, I perforated the cork, so as just to admit the nozzle of a small funnel, when it is thrust in pretty hard. I then tie this cork in the mouth of a bladder large enough

enough to contain at least half as much air as the phial. But if I use dephlogisticated air instead of common air, for the decomposition of the nitrous air, the bladder ought to contain about five times as much as the phial.

If the process be made with common air, I convey into the bladder half as much nitrous air as the phial can contain; and taking care to dispose it so that no water that may be in the bladder can get into the phial (which is not difficult to manage) I withdraw the funnel, and put the cork of the bladder into the phial; then pressing the bladder a little at first, in order to force a little of the nitrous air out of the bladder into the phial, the effervescence immediately begins, and of course the diminution of the air in the phial; the consequence of which is, a demand for more nitrous air from the bladder, to supply the place of that which had disappeared. This again produces a stronger effervescence; and by this means, after the redness has begun to appear in the phial, all the nitrous air will presently be drawn out of the bladder, and be decomposed within the phial; and all the acid that it forms will be imbibed by the water in the phial, or by any other substance that may be placed there for the purpose.

Before I endeavoured to ascertain, by this means, the precise quantity of nitrous acid furnished by a
given

given quantity of nitrous air, I was willing to try how strongly I could impregnate a quantity of water with the nitrous acid in this manner; and I found that I could make four ounce measures and a half of water receive all the acid that could be supplied by 300 ounce measures of nitrous air; and that then the water was become quite blue. But water impregnated with the nitrous acid vapour either in this manner, or from the solution of metals in strong nitrous acid, adheres to it very slightly, and can hardly be prevented from making its escape. It is impossible to pour water thus impregnated from one vessel to another, but the copious red fumes which issue from it, even when the water is quite colourless, shew that it suffers a great loss of its acid. A quantity of the impregnated water which I got in this experiment, equal in bulk to a quantity of distilled water weighing four penny weights, being poured upon copper, yielded only one ounce measure and a quarter of nitrous air. The same quantity of the strong spirit of nitre made in the common way will yield twelve or fifteen ounce measures. In order to make my principal experiment with more accuracy, I was taught by this to think it necessary to provide such a quantity of water as would be but slightly impregnated, that the loss by evaporation might be the less in proportion.

In order to compare the quantity of nitrous acid formed by a given quantity of nitrous air, with the quantity of acid contained in an equal bulk of red nitrous vapour procured by the solution of bismuth, I saturated a quantity of water in the method described above, with as much of the acid as was contained in a pint phial of nitrous air; and having filled the same phial with the red vapour, I put into it the same quantity of water, to imbibe that vapour; and then pouring all the water impregnated with nitrous acid from the nitrous air, and afterwards that which was impregnated by the nitrous vapour, upon equal quantities of copper, in a phial fitted with a ground stopper, I collected the nitrous air that they yielded, and found them to be exactly the same; and this was one seventh of the quantity of nitrous air by which the acid had been formed; so that, provided there was no loss of the acid in the process (and I am pretty well persuaded that the loss could not have been at all considerable) it may be inferred from this experiment, that the nitrous acid which is detained in the *salt* made by the solution of copper, is six times as much as can be formed by the nitrous air procured by that solution.

In order to ascertain what quantity of nitrous vapour I had got in the phial that was filled by means of the solution of bismuth, I filled the phial
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a second time in the same manner, as nearly as I could determine (but this estimate is attended with a good deal of uncertainty) and admitting water to it, I found that a little less than one half entered it; that proportion of its contents having been occupied by the nitrous vapour, and the remainder by the common air diffused through it. Consequently, nitrous air does not form quite half so much nitrous acid as the same bulk of red nitrous vapour from the solution of bismuth; supposing that as much red vapour was contained in the phial as can, at a medium, be thrown into it. But two phials will look almost equally red, so as hardly to be distinguished by the eye, when one of them does not contain much more than half as much as the other.

The saturation of water with nitrous acid from nitrous air, by means of dephlogisticated air, makes a pleasing experiment, on account of the great quantity of nitrous air decomposed by this means at one process, the quickness with which the decomposition is made, and the visible effect of the sudden impregnation on the water. For the surface of it instantly becomes, as it were, oily, descending in waves from the top to the bottom of the water, while nitrous air issues plentifully from the bottom and sides of the vessel; a most remarkable phenomenon, of which a full account will be given in a proper place.

SECTION V.

Of Water in nitrous Air.

THAT water enters into the composition of nitrous air, is not improbable, because it is procured in so great abundance from pure water impregnated with phlogisticated nitrous vapour, and also from its not being procured from copper, and other metals, except in a very diluted solution of the nitrous acid.

That water enters into the constitution of every kind of air I also supposed, because it certainly does into that of *inflammable, fixed, and dephlogisticated* air. That *nitrous* air contains water, I have lately found from the iron that is heated in it becoming a proper finery cinder.

I had found before, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogisticated air. Having since that time repeated this experiment, and afterwards heated the iron, which was by this means increased in weight, in inflammable air, the iron lost its additional weight, and water was copiously produced, as in the same process with finery cinder, or, as I sometimes call it, scale of iron.

As

As nitrous air may be deprived of its water, and become phlogisticated air by heating iron in it, I find that it undergoes the same change by being repeatedly transmitted through hot porous earthen tubes, through which I some time ago discovered that vapour will pass one way, while the air contiguous to the heated tube will pass the other.

I first tried this process with turnings of iron in the tube, by which means the iron was readily converted into finery cinder; but afterwards I found that the same change was produced in the nitrous air by the hot tube only. The two bladders which I made use of in this experiment (and by the alternate pressure of which I made the air contained in them pass through the hot tube) became *red*, just as any bladder does that is filled with nitrous air, and then exposed to the influence of the atmosphere till it becomes phlogisticated air, as may be seen in my former experiments. In this manner I now always treat the bladders in which I make experiments on air. It prevents them from putrefying, and gives them a firmness of texture similar to tanning.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. I heated five grains of charcoal of copper in eight ounce measures of nitrous air, till it was increased to ten

ounce measures, and the charcoal had lost one grain. Examining the air, I found about one fifth of it to be fixed air, and the remainder phlogisticated. It seems, therefore, that nitrous air consists of water, and something that may be called the basis of nitrous acid, or that substance which, when united to dephlogisticated air, will make nitrous acid; and this seems to be pure phlogiston, since it is found, as the preceding experiment shews, in the purest inflammable air. May we not hence infer, that the nitrous is the simplest of all the acids, and perhaps the basis of all the rest?

It is evident, that more water than enters into the composition of nitrous air is necessary for the change of it into what I have called *dephlogisticated nitrous air*, because the contact of iron will not, without water, produce that change in it.

Though fixed air, as I have shewn, contains water as well as nitrous air, it cannot be deprived of it, and be decomposed, by the same means; for I have heated iron in it by a burning lens, and have also made it pass repeatedly through a hot earthen tube containing turnings of iron, without producing any change in it.

That nothing is necessary to the formation of nitrous air besides phlogisticated nitrous acid and water, is evident from the production of it by the
impreg-

impregnation of pure water with phlogisticated nitrous vapour, formed by the rapid solution of bismuth; an experiment which I have mentioned before. However, to make it in a more unexceptionable manner, I interposed a glass vessel between that in which the solution was made, and that in which the water to be impregnated with the phlogisticated vapour was contained; that whatever of the solution was distilled over by the heat of the process (which evidently occasioned the dropping of a blue liquor from the end of the tube through which the vapour was delivered) might be prevented from reaching the water. In these circumstances, however, when nothing but the dry phlogisticated vapour could enter the water, it began to sparkle, and yield nitrous air very copiously, as soon as it had acquired a blue tinge from the impregnation. This is a very striking experiment to all persons when they have seen it the first time.

Nitrous air is also produced by pouring a highly coloured, or phlogisticated, nitrous acid into pure water, in which no metal or earthy matter is any way concerned.

SECTION VI.

*Of the Decomposition of nitrous Air by heating in it
Iron or Charcoal.*

NITROUS air seems to contain more water than phlogisticated air. For when iron is heated it, which, by getting water in this process, becomes finery cinder, the nitrous air is converted into phlogisticated air, as will be seen more distinctly hereafter. Hot charcoal has a similar effect upon it.

I dropped a piece of red hot charcoal into a phial of nitrous air, and immediately inverting it in a basin of mercury, the air was presently reduced to one fifth of the whole. Thus it continued two months, without any sensible change; after which I found that the air that remained unabsorbed did not affect common air, nor did the air that was emitted by the charcoal, when it was plunged in water; so that, in both these cases, the air seems to be intirely deprived of its peculiar properties, and to become mere phlogisticated air.

Having heated *iron* in nitrous air, I proceeded to heat in the same air a piece of *charcoal*, not
long

long after it had been subjected to a strong heat covered with sand. The sun not shining immediately after the charcoal was introduced into the vessel of air (through the mercury, by which it was confined) part of the air was absorbed; but on heating the charcoal the quantity was soon increased. Having continued the process as long as I thought necessary, I examined the air, and found it to be about as much as the original quantity of nitrous air, but it was all phlogisticated air, extinguishing a candle, and having no mixture of fixed air in it. At one time, when I dipped a lighted candle into it, I thought there was an appearance of its containing something inflammable, but it was in the slightest degree imaginable. There was, however, evidently something inflammable in the air expelled by water from this charcoal afterwards.

When I heated *charcoal of copper* in nitrous air, there was a small addition to it, of perhaps a twentieth part; and being examined, it was found to contain two sevenths of its bulk of fixed air, and the rest was phlogisticated. This change in four ounce measures of the air was produced by the loss of between one fourth and one half of a grain of the charcoal, and the copper was evidently revived.

That fixed air should be produced in this experiment, and not in the preceding, is rather extraordinary.

dinary. In this circumstance it resembles the experiment on the heating of charcoal of copper in dephlogisticated air.

It is something remarkable that heating the *scales of iron* in nitrous air, has the same effect as heating iron itself in it, but then much more *time* is requisite to produce the same effect.

I heated a quantity of scales of iron in nitrous air till two ounce measures and seven tenths were reduced to one ounce measure and thirty five hundredth parts, and then found it to be wholly phlogisticated air, extinguishing a candle, and neither affecting common air, nor being affected by fresh nitrous air. During the process the surface of the scales on which the focus of the lens fell were affected in a very peculiar manner, as if they were superficially melted, the parts being in a state resembling that of ebullition, which continued a long time on the same spot. No moisture was produced in the process, as is the case when these scales are heated in inflammable air.

When I put the scales which had been used in this experiment into diluted oil of vitriol, it became white like milk, and very little air was produced, even by means of heat. I got, however, a few bubbles; so that with good management I could just be sure that it was not fixed air, that it did not affect nitrous air, nor was affected by it.

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SECTION VII.

Of volatile Alkali procured by Means of nitrous Air.

HAVING, for the purpose of producing a large quantity of that kind of nitrous air in which a candle burns with an enlarged, or with a vivid flame, filled a large jar with pieces of iron wire, and having repeatedly poured upon them a diluted solution of copper in the nitrous acid, at length a thick incrustation was formed upon them; and having no occasion to make use of the jar for several months, I took no notice of it till I found the jar was burst by the swelling of that saline incrustation.

The substance of this matter was generally red, being the calx of iron; but there was mixed with it a quantity of *green matter*, which, when broken had a strong smell of volatile alkali. I then doubted whether this arose from any of the materials I have mentioned, or from something else which had got into the jar, unknown to me. If the former were the case, which, however, at that time, I could hardly suppose, I thought it to be not a little remarkable; but I have since had another opportunity

tunity of observing the same fact; having examined a second jar filled with iron wire, which had been treated in the same manner, and found the same strong smell of volatile alkali. Also I now the less wonder at this fact, which puzzled me so much at the first, as I find, in Mr. Keir's very valuable notes to his translation of Mr. Macquer's chemical dictionary, that volatile alkali has been found in many earthy substances, and amongst others, in *rust of iron distilled*.

It should seem that, in general, the calces of metals contain less phlogiston than the metals themselves; and for this reason I was originally led to conclude, that nitrous air exposed to iron, which is evidently turned to *rust*, or a *calx* in it, had received phlogiston from the metal; and I therefore termed the nitrous air that had been so treated *phlogisticated nitrous air*. I now think it most probable that this rust of iron contains more phlogiston than the iron itself, and that the nitrous air, in which, after this process, a candle burns better than in common air, is properly termed a *dephlogisticated nitrous air*, having parted with its phlogiston to the iron.

Having found that iron which had long rusted in nitrous air, gave out a strong smell of volatile alkali, I afterwards repeatedly observed the same, and also some other curious particulars relating to the experiment.

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In all the cases in which I had before observed this smell of volatile alkali, the iron had been exposed to the nitrous air a very long time. But this effect is produced much sooner by putting the iron into a weak solution of copper in the nitrous acid, such as remains after making nitrous air, and such as I now commonly use in order to procure dephlogisticated nitrous air. A phial containing some of this iron, which had been used not more than once for this last mentioned purpose, having been kept close corked about two months, was accidentally broken; when some pieces of the iron were found covered with a green rust, and these had a strong smell of volatile alkali.

Time, however, is requisite to produce this effect. I put some of this iron (which was of the turnings) moist, from a solution of copper in the nitrous acid, into a phial; and stopping it close, left it to stand with its mouth immersed in water. At that time it had no smell, nor had it any a month afterwards; but examining it again after two months, the smell of volatile alkali was pretty strong.

Having, in an early period of my experiments, been much struck with the change of nitrous air, in consequence of the contact of iron, I kept several large bottles filled with small pieces of iron, which I occasionally supplied with nitrous air.

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The bottles should be strong ones, because the iron in rusting swells, and is apt to break the phials, though I do not know that this ever happened except when the iron was quite dry. The consequence of exposing nitrous air in this manner had always been, in the first place, a diminution of something more than one third of the quantity of it, in which state a candle would burn in it with an enlarged flame; and when well washed in water it would be farther diminished about one half, when the remainder would be phlogisticated air. I had never observed any *increase* of air in these bottles, though some of them had been used a long time. But having neglected one of them about a year (though the mouth of it had always been kept immersed in water) I casually observed, that bubbles of air occasionally issued from it. This surprised me very much, as I imagined it had not been half filled with air; and placing a vessel to receive these bubbles, in about a week or ten days, I got about the quantity of an ounce measure. It contained no fixed air, but was all strongly inflammable, and when I analyzed it, it appeared to be of that kind into which alkaline air is converted by heat.

It seems as if the phlogisticated air which is ultimately produced in this process, had been decomposed in these circumstances, so as to form this
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inflammable air, but on what principle this should take place, I cannot imagine.

In order to make a comparison between the inflammable air produced from this bottle, and that which had been made from alkaline air, I exploded equal measures of each of them with equal quantities of the same dephlogisticated air, the standard of which was 0.3. When I made the explosion with the inflammable air from alkali, the first diminution was to 1.2, and with that from the bottle it was 1.1. Of the former residuum the standard was 1.1; and of the latter 0.98. Neither of them contained any portion of fixed air. On the whole, the resemblance between these two kinds of air was so great, and the difference between them so small, that they may be concluded to have had the same origin, and the same constitution.

SECTION VIII.

General Observations on the constituent Principles of nitrous Air.

THERE is no kind of air, the constituent principles of which seemed to be more clearly ascertained than those of nitrous air, by those philosophers who admitted the doctrine of phlogiston. We took it for granted, that it consisted of nitrous acid and phlogiston. In an early period of my experiments I endeavoured to ascertain the quantity of acid contained in it. Mr. Kirwan has done the same, and Mr. Cavendish also has considered it as containing a very concentrated nitrous acid. I had no doubt on the subject till I read the work of Mr. Metherie, who asserts that nitrous air contains no proper nitrous acid, but only one of the elements of it, the other being dephlogisticated air, which had before been considered by Mr. Lavoisier as the principle of all *acidity*.

Among other observations in support of his assertion Mr. Metherie has the following. Nitrous air burned together with inflammable air, produces no nitrous acid, p. 145. Though nitrous air is obtained from

from a solution of mercury in nitrous acid, almost all the acid is found in the solution, p. 146. Nitrous air absorbed by marine acid, does not make aqua regia, p. 153. He is of opinion that a small portion of the nitrous acid being decomposed, furnishes a pure air, so altered that, uniting with inflammable air, it changes it into nitrous air, p. 150.

As to this *hypothesis* of Mr. Metherie, I cannot say that I well understand it; but, being struck with his *observations*, I began to review the experiments which I had formerly made on this kind of air, and could not recollect any of them in which it appeared that real nitrous acid was produced, except when nitrous air was combined with dephlogisticated air, besides the experiment in which it was decomposed by the electric spark, which furnishes a strong objection to this hypothesis. In every other view Mr. Metherie's theory appeared to me not improbable, and led me to make the following experiments, which though not decisively in favour of it, may deserve consideration. They will at least shew that, if there be all the elements of nitrous acid in nitrous air, they must, together with an additional portion of phlogiston, exist in the phlogisticated air that is formed in the processes.

When nitrous air is decomposed by iron, or by a mixture of iron and sulphur, I have observed that
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the water over which the process is made, acquires no acidity by it; but I had supposed that all the acid had been seized by the *iron*. Having by me a considerable quantity of this iron, which had been reduced to a perfect rust in nitrous air, and which I knew must have imbibed more than its weight of this air, I thought that I might get the acid out of it by distillation, if it contained any. But a quantity of this rust of iron, carefully distilled in an earthen retort, yielded neither nitrous air, nor nitrous acid, at least in any quantity that could favour the common hypothesis. For besides the *air* (of which an account will be given in its proper place) some ounces of this rust yielded only a few drops of liquor, which was not sensibly acid to the taste, and when poured into a little water, tinged blue with the juice of turnsole, barely turned it red. But so slight a degree of acidity as this might have come (and I have no doubt did come) from the union of nitrous air with those bubbles of common air which could not be excluded from the vessels containing the iron, in the many times in which I changed the air, when I supplied them from time to time with fresh nitrous air; so that the great mass of nitrous acid which ought to have been deposited in this iron must have been concealed in the phlogisticated air, which is generally about one fourth of the original quantity of nitrous air.

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This brown rust (become so by exposure to the air, which it imbibes) became black by the expulsion of its air and water, and exactly resembled that iron ore which is found in the form of a black sand.

I then endeavoured to decompose nitrous air by heating iron in it with a burning lens; and in this process I succeeded much beyond my expectation: For the air was presently diminished in quantity, while the iron became of a darker colour, was sometimes melted into balls, and gained considerable weight. It had no appearance, however, of containing any nitrous acid, and yet water tinged with the juice of turnsole introduced into the vessel, and agitated in it after the process, never changed its colour.

In the first experiment the original quantity of nitrous air was diminished to about one third, and after this it was increased. But then I found that when I had got to the extreme of diminution, there began to be an addition made of inflammable air from the iron; for this was evidently so. It was also observable that there was either some pure air, or dephlogisticated nitrous air, in what remained of this process. For when a measure of this residuum was mixed with an equal measure of common nitrous air, they were reduced to 1.7. I suspect, however, that this was owing to a quantity of dephlogisticated nitrous air being formed by this process,

cess, as it is similar to the rusting of iron in nitrous air.

In another process of this kind, I reduced six ounce measures of this air to three, and this was wholly phlogisticated air, extinguishing a candle. I also heated iron in seven ounce measures and a half of nitrous air, till it was reduced to 3.7 measures, when it appeared to be perfectly phlogisticated air. The iron had then gained two grains and a quarter in weight, and at this time I observed that there was not the least appearance of *moisture* produced in the course of the experiment.

Then recollecting that, in my former experiments, Homberg's pyrophorus had taken fire in nitrous air, I made a quantity of this substance, and repeated the experiment with the greatest care. But when nitrous air was decomposed by this means, I saw no reason to think that any nitrous acid had been formed. In this process the nitrous air is readily diminished to about one half of its bulk, after which nothing nitrous is found in it, but a portion of dephlogisticated nitrous air; as appears by its being much more readily absorbed by agitation in water than nitrous air is. But neither the air, nor any thing that can be expelled from the pyrophorus, discovers any sign of containing nitrous acid.

Having saturated a quantity of pyrophorus with nitrous air in an earthen tube, which by this means became

became very hot; I admitted water to it; and distilling the pyrophorus with this water, the liquor that came from it would not change the colour of the juice of turnsole; and the air which came afterwards was of the same kind with that which is always expelled from pyrophorus after it has been burned in the open air, viz. partly fixed air, and partly inflammable.

I shall just mention some farther particulars of another experiment of this kind, though it was made long before I had the views which led to those now mentioned. Having saturated about two ounce measures of Homberg's pyrophorus with nitrous air, I observed that it had imbibed about four times its bulk of the air. This pyrophorus was of a kind that did not burn readily in the nitrous air, or I believe in common air, without external warmth. Having withdrawn it from the nitrous air, it took fire again, but with more external heat. Then putting it into an earthen retort, I expelled from it ninety ounce measures of air, a very small proportion of which was fixed air. The former portions of it extinguished a candle, and were of the standard of 1.7, and the latter of 1.55, burning with a lambent blue flame. Three days after this, being taken out of the retort (the mouth of which had been all the time immersed in mercury) a small part of it took fire. I observed it then weighed 416 grains, and the next morning it weighed 438 grains.

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There is something very remarkable in this substance burning equally well in two such very different kinds of air as nitrous and dephlogistigated.

In my former experiments I found that the juice of turnsole became red by being impregnated with nitrous air. I found, however, that this must have been owing to some extraneous acid that was mixed with it. For when I took nitrous air that had stood long in water, and transferred it into a vessel of mercury, I impregnated with it water tinged with the juice of turnsole, without producing any change in its colour.

I have also observed that alkaline air admitted to nitrous air, made a whitish cloud in it. But this must have been owing to the same cause; the nitrous air in that experiment having been probably fresh made. For in the nitrous air which had been long confined by water, the mixture of alkaline air, did not produce the least cloud. I perceived, however, that after the two kinds of air has been together a day or two, there was a small quantity of a very deeply tinged blue liquor in the space between the mercury and the glass; which shows that there was copper dissolved in the nitrous air, and that it continued there after the extraneous acid was separated from it. On this copper the volatile alkali had seized.

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Notwithstanding these results, when the electric spark was taken in the very same nitrous air, with which the preceding experiments were made, the juice of turnsole by which it was confined, presently became red. This circumstance deserves to be examined with care, as it seems to be decisively in favour of the nitrous air containing all the necessary elements of nitrous acid.

P A R T IV.

OF DEPHLOGISTICATED NITROUS AIR.

SECTION I.

Of the Discovery of dephlogisticated nitrous Air, by exposing Iron to nitrous Air.

AS fixed air united to water dissolves iron, I had the curiosity to try whether fixed air alone would do it; and as I then supposed nitrous air to be of an *acid* nature, as well as fixed air, I, at the same time, exposed a large surface of iron to both the kinds; first filling two eight ounce phials with nails, and then with quicksilver, and after that displacing the quicksilver in one of the phials by fixed air, and in the other by nitrous air; then inverting them, and leaving them with their mouths immersed in basons of quicksilver.

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In these circumstances the two phials stood about two months, when no sensible change at all was produced in the fixed air, or in the iron which had been exposed to it, but a most remarkable, and most unexpected change was made in the nitrous air; and in pursuing the experiment, it was transformed into a species of air, with properties which, at the time of my first publication on this subject, I should not have hesitated to pronounce impossible, *viz.* air in which a candle burns quite naturally and freely, and which is yet in the highest degree noxious to animals, insomuch that they die the moment they are put into it; whereas, in general, animals live with little sensible inconvenience in air in which candles have burned out. Such, however, is nitrous air, after it has been long exposed to a large surface of iron.

It is not less extraordinary, that a still longer continuance of nitrous air in these circumstances (but *how long* depends upon too many, and too minute circumstances to be ascertained with exactness) makes it not only to admit a candle to burn in it, but enables it to burn with an *enlarged flame*, by another flame (extending every where to an equal distance from that of the candle, and often plainly distinguishable from it) adhering to it. Sometimes I have perceived the flame of the candle, in these circumstances, to be twice as large as it is naturally, and sometimes not less than five or six times larger;

and yet without any thing like an *explosion*, as in the firing of the weakest inflammable air.

Nor is the farther progress in the transmutation of nitrous air, in these circumstances, less remarkable. For when it has been brought to the state last mentioned, the agitation of it in fresh water almost instantly takes off that peculiar kind of inflammability, so that it extinguishes a candle, retaining its noxious quality. It also sometimes retains its power of diminishing common air in a very great degree.

Nitrous air treated in the manner above mentioned, is diminished about one fourth by standing in quicksilver; and water admitted to it will absorb about half the remainder; but if water only, and no quicksilver, be used from the beginning, the nitrous air will be diminished much faster and farther; so that not more than one fourth, one sixth, or one tenth of the original quantity will remain.

The water which has imbibed this nitrous air exposed to iron is remarkably green, also the phial containing it becomes deeply, and, I believe, indelibly tinged with green; and if the water be put into another vessel, it presently deposits a considerable quantity of matter, which when dry appears to be the earth or ochre of iron.

Instead of using *iron*, I introduced a pot of *liver of sulphur* into a jar of nitrous air, and presently found, that what I had before done by means of
iron

iron in six weeks, or two months, I could do by liver of sulphur in less than twenty four hours, especially when the process was kept warm.

When the process is in water with iron, the time in which the diminution is accomplished is exceedingly various; being sometimes completed in a few days, whereas at other times it has acquired a week or a fortnight. Some kinds of iron also produced this effect much sooner than others, but on what circumstances this difference depends I do not know.

SEC.

SECTION II.

Of dephlogisticated nitrous Air, produced by the Solution of Metals in nitrous Acid.

THE solution of iron in spirit of nitre is known to produce nitrous air ; but when all the nitrous air is produced in this manner, without foreign heat, if a candle be applied to the solution, more air will be procured ; and this will be possessed of the peculiar kind of inflammability mentioned in the preceding section. But I first met with this air by dissolving iron in water, first impregnated with vitriolic acid air, and then with nitrous vapour, as will be mentioned in its proper place. The first produce of this solution was nitrous air ; but applying the flame of a candle, when no more air could be got without it, air was produced in which a candle burned with a natural flame.

It was upon observing this, that I tried the same experiment after the termination of the common solution of iron in spirit of nitre ; when, by this means, I got, by a direct process, such a kind of air as I had brought nitrous air to be by exposing it to iron, or liver of sulphur ; for on the first trial a candle

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burned

burned in it with a much enlarged flame. At another time the application of a candle to air produced in this manner, was attended with a real, though not a loud *explosion*; and immediately after this, a greenish coloured flame descended from the top to the bottom of the vessel in which the air was contained. In the next produce of air from the same process the flame descended *blue* and very rapid, from the top to the bottom of the vessel.

When nitrous air is produced from iron, the quality of it, I doubt not, is always the same, though there is a case in which I do not wonder that some persons have been deceived; having got phlogisticated air when they expected nitrous; not considering, that exposure to a large surface of iron decomposes nitrous air, as my former experiments shew; changing it at first into a species of air in which a candle will burn, and then into phlogisticated air. This process, however, required a considerable time; but the following experiment shews that this effect may be produced very soon.

I filled an eight ounce phial with small nails, then with water, into which I put a very small quantity of nitrous acid, just enough to make it produce air; and then some was yielded in which a candle went out. Also this acid water poured off from the iron, gave a considerable quantity of air, in the heat

heat of boiling water, and this was all phlogisticated air.

Using more spirit of nitre, I observed, that, though the production of air was pretty copious, as was manifest by the bubbles formed at the bottom of the phial, and rising to the top, there was no increase of the whole quantity of air in the phial. Examining the air in this state of the process, I found that it had very little power of diminishing common air, and that a candle burned in it with a vivid flame, which is the intermediate state of this air, before it becomes phlogisticated air, I imagine, therefore, that in all these cases, a proper nitrous air is first produced, and that it is afterwards, by means of the iron to which it is exposed, changed into that species of air in which a candle can burn, and lastly into phlogisticated air, which extinguishes a candle.

I have also procured this kind of air in a direct process by the solution of *zinc* and *tin*.

I completely saturated a quantity of spirit of nitre with zinc. This solution, while it was hot was fluid, but when it was cold it resembled a whitish jelly. Distilling this jelly to dryness, in a glass vessel, and receiving the air that came from it in three parts, I observed that the first was the common air in the phial, very little phlogisticated; which

which shews that, after the common solution of this metal, without additional heat, very little phlogiston remains in the solution. The second portion was twice as good as common air, and the third was as pure as any that I had ever met with.

At another time that I repeated this experiment, I received the air in more portions; but the general result was the same, except that between the phlogistified and the pure air, there was a quantity of that kind of nitrous air in which a candle burns with an enlarged flame. That it was precisely the same kind of air that was procured by the direct solution of zinc described above was evident, because after that *vapour* (which is always the cause of this peculiar phenomenon) was washed out of it, it neither affected common air, nor was affected by nitrous air; being the same thing with that which I have termed *phlogistified air*, and which is always the basis of this peculiar kind of nitrous air.

This remarkable fact seems to prove that this species of air contains less phlogiston, in proportion to its bulk, than phlogistified air, and consequently that the vapour which is the cause of this peculiar property, and which is readily imbibed by water, partakes chiefly of the nature of the nitrous acid. For, in all these processes, every produce of air has less

less and less phlogiston, till we came to the last, which contains the least possible. Also this kind of air coming between the phlogisticated and the pure air seems to shew that, if that which constitutes the enlarged flame be phlogiston contained in the air itself (and indeed it can hardly admit of any other supposition) it must be in a state not perfectly combined with that nitrous acid vapour. We see also that this diffusion of phlogiston, and the loosening of its connexion with the other constituent parts of the air is that state of it which immediately precedes its total disappearing. For immediately after this kind of air, we find the next produce to have no phlogiston at all.

Lest this intervention of the dephlogisticated nitrous air between the other kinds of air, and the purest of all, should have been occasioned by some accident, I carefully repeated the experiment, and found it in the same place a second time. Having dissolved a quantity of zinc in a quantity of one fourth spirit of nitre and three fourths water, distilling the solution to dryness, and receiving the produce in four portions, the first was only the common air expelled from the phial, and not sensibly injured; the second was *dephlogisticated nitrous air* (as we may call that kind of nitrous air in which a candle burns with an enlarged flame) the third was

was dephlogisticated air, about four times as pure as common air; and the fourth was as pure as any that I have ever examined.

In order to ascertain the quantity of air produced by the solution of zinc in spirit of nitre, in a more satisfactory manner than I had been able to do at first, I suspended a lump of zinc within the mouth of a large phial, in the bottom of which was a quantity of strong spirit of nitre; and when I had placed them under the receiver of an air-pump, I carefully exhausted the air, and then, by means of the apparatus for a collar of leathers, I let down the zinc into the acid. Immediately upon this there was a most prodigious effervescence; but, the vessel into which it was made being very large, no part of the fluid escaped out of it.

By this means I got a very considerable quantity of air; and when I examined it, it appeared to be the very same thing with nitrous air exposed to iron or liver of sulphur, and such as I had got in the last-mentioned experiment by means of heat from the direct solution of iron; and the great heat attending this effervescence might answer the same purpose as the foreign heat of the candle in the case of the iron, viz. expelling the acid with more force, and throwing it into a state into which it was not strictly combined with phlogiston.

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The air thus procured diminished common air about half as much as nitrous air would have done; owing, I suppose, to half of it being properly nitrous air, and the remainder *nitrous vapour* uncombined with phlogiston, and diffused through the nitrous air. One third of this air was readily absorbed by water, which is another circumstance in favour of its containing an acid vapour; and a candle burned in it with an enlarged flame, almost twice as large as its own proper flame, very vivid, and surrounded with another flame of a blue colour. With a very slight agitation in water, this air extinguished a candle; the water having, by this means, probably, absorbed the acid vapour that was diffused through it.

In order to ascertain the *quantity* as well as the quality of the air produced in this manner, I dissolved four penny-weights of zinc in very strong spirit of nitre, contained in a glass phial with a ground stopper, when I got about eight ounce measures of air, which I took at twice; but neither part of this produce either affected common air, or was affected by nitrous air; and though part of it was absorbed by water, it did not make lime water turbid. Also no candle would burn in it. But I did not examine this air till about two hours after it was generated, when a considerable part of it was absorbed by water. Had I made a trial of it

it immediately after it was produced, and before any part of it had been absorbed by water, I make no doubt but a candle would have burned in it. For when I repeated the experiment, and tried both parts of the produce immediately, a candle burned with an enlarged flame in the former, and with a natural flame in the latter.

At other times, however, I have got nothing of the peculiar kind of air above-mentioned, but only strong nitrous air from seemingly the same process; and I was not able to determine on what circumstances the different results depended. I once thought that I had got this dephlogisticated nitrous air, when I used a very large quantity of spirit of nitre, much more than was necessary to dissolve the zinc, but at other times this expedient failed me. When I had taken the air at different times, I found the last produce more nitrous than the first. But I once divided the produce into five parts, and found all of them nearly equally, and all very strongly nitrous.

It will be seen that in the solution of *tin*, which I find to contain very little phlogiston, this air is always produced, as will appear when I come to recite the phenomena attending the solution of this metal in the nitrous acid, among other metals that yield dephlogisticated air.

When, after all the uncertainty of the preceding trials, I used only a very weak spirit of nitre in the solution of zinc (with a view to produce air in about the same quantity as it had been yielded by means of the water impregnated first with vitriolic acid air and then with nitrous vapour) I got no other than this kind of air in which a candle burned with an enlarged flame; and the air was of the very same kind from the beginning to the end of the process.

I observed that when I had just dissolved all the metals in spirit of nitre, *tin* yielded nitrous air as well as the rest. But had I prosecuted the experiment, I should have found that the proper nitrous air which it yields is in very small quantity, and that the greatest part of the produce is that kind of nitrous air in which a candle burns with an enlarged flame. This metal also yields no fixed air; but several of the circumstances attending this solution, being pretty remarkable, and I having varied then pretty much, it may not be amiss to recite some of them.

Tin did not dissolve so as to yield air in strong spirit of nitre, but was affected by it in the manner that Mr. Macquer describes. — With an equal quantity of water and spirit of nitre, it dissolved with great violence, and five penny-weights fix grains yielded about fifteen ounce measures of air, about
one

one fifth of which was absorbed by water, being the nitrous vapour, so often mentioned above, diffused through the air. Before this part was washed out, a candle burned in the air naturally; but when this part had been absorbed by the water, a candle went out in it. The former part of the produce, which was three fourths of the whole, was but slightly nitrous; for two measures of common air and one of this occupied the space of two measures and a half; and the last part was proper phlogisticated air, neither affecting common air, nor being affected by nitrous air. Had I taken the produce in small portions, the first produce would, I doubt not, have been proper nitrous air, as I had first found. I particularly observed that no part of this air, though readily absorbed by water, made lime water turbid, so that it contained no fixed air.

After this, distilling the residuum to dryness, in a glass phial with a ground stopper, I got about twelve ounce measures of air, in four parts; of which a portion of each, and especially at the first, was readily absorbed by water, but without making lime water turbid. When this ingredient was washed out, the air which had lodged within the phial was phlogisticated. Then the first portion of the proper produce of the materials was com-

mon air, the second twice as good, and the remaining portions were exceedingly pure.

Putting more spirit of nitre upon the residuum, I got only about twice its bulk of air, and part of this was fixed air; and in all respects this process seemed to resemble that with flint. After this I weighed the residuum, and found it to be five pennyweights two grains.

The phenomena attending the solution of *tin* being so very remarkable, I repeated it several times, with a considerable variation of circumstances; and the following appeared to me to be worth reciting. I dissolved a quantity of tin in a quantity of spirit of nitre, diluted with an equal quantity of water; when the solution was at first very slow, but afterwards very rapid. In the course of the process I several times took a small quantity of the air to lime water, but it was not made in the least degree turbid, though a small part of the air was absorbed by it.

I received the whole produce in four parts, and found that the first was so far nitrous, that two measures of common air and one of this occupied the space of two measures and a half; the second was a little more nitrous; in the third the same measures occupied the space of two measures and a quarter; but in the last the air was less nitrous,
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the same mixture occupying the space of two measures and three fourths. Towards the middle of the process, the air was very white, and continued so a long time. N. B. In the first of the portions of air above-mentioned a candle burned very bright, and with a crackling noise; in the second and third it burned with a large flame, and very silently, a blue flame being very distinctly perceived surrounding the central white flame. The fourth extinguished a candle.

When I could get no more air without additional heat, I applied the flame of a candle, and afterwards of two candles; and then took about half as much air as I had got before, in six portions; of which the first was nitrous, in about the same degree as the last part of the former produce; the second was so much nitrous, that two measures of common air and one of this occupied the space of two measures and one sixth; the third hardly affected common air at all; the fourth was twice as good as common air; and the fifth and sixth portions, which were four times the quantity of the preceding, were highly dephlogisticated. N. B. The third, fourth, and fifth portions were exceedingly turbid; but the last was quite clear, coming very slowly.

In these two processes, with and without foreign heat, we see a pretty regular gradation from the most impure to the most pure kind of air,

SECTION III.

Of dephlogisticated nitrous Air produced in the Diminution of nitrous Air by Iron Filings and Sulphur, and also by Liver of Sulphur,

THE first remarkable diminution of nitrous air that I observed was occasioned by the fermentation of iron filings and sulphur, made into a paste with water. This process is attended with much heat, the diminution of the air is exceedingly rapid, and whenever I examined the air that remained, it always appeared to be simply phlogisticated air, neither affecting common air, nor being affected by nitrous air, and extinguishing a candle. And in this last-mentioned property it differed from nitrous air diminished by iron only, or by liver of sulphur, in which a candle burned with an enlarged flame before it was agitated in water. I have since, however, observed that nitrous air diminished by iron filings and sulphur does not really differ from that which is diminished by the other processes; but that this process being made in a large quantity of water, either the superfluous nitrous acid vapour, the superfluous phlogiston,

giston, or both, were always absorbed before the experiment was made. This I discovered by repeating the process with more care and attention, in the following manner.

Having introduced a pot of iron filings and sulphur into a large jar of nitrous air, I examined the state of the air in all the stages of its diminution, from the time that the fermentation began till it could be diminished no more by that process. In order to get a small quantity of the air, without moving the jar, or disturbing the apparatus contained in it, I fastened a small phial, or a piece of glass tube, to the end of a stiff wire; and filling it with water, I put it up into the vessel, with its mouth downwards; when, the water running out, it would necessarily be filled with the air of the jar, which I could then with the same ease withdraw, and examine.

Proceeding in this manner, I found that, in the last stage of the diminution of this air, and not before, a candle burned in it with an enlarged flame. This process, therefore, exactly resembles that with iron only or liver of sulphur, only that in this case the air must be examined very soon, before the water can have had an opportunity to act upon it. For when I tried part of the same air the next morning (when, without any agitation, part of it had been absorbed by water) it extin-

guished a candle; having, in the mean time, become mere phlogisticated air.

I transferred another quantity of nitrous air from quicksilver, in which the process was made, to lime water, but it did not make it in the least turbid. Indeed, but little of this air was absorbed by water, and a candle burned in it only naturally, without any enlargement of the flame. It had been diminished to about one third of its bulk in the quicksilver.

I shall now proceed to note other phenomena attending this process, which the philosophical reader will easily perceive may be of great use in the analysis of the species of air, as well as help to explain the process itself. In order to determine whether the *acid*, or *phlogiston*, of the nitrous air (for I then thought it contained both) had been seized upon in this process, I made it over a quantity of very pure water; thinking that, if it should acquire any acidity, it would shew that the phlogiston only had been seized; but that if it should not have become acid, it would appear that the decomposition had been effected by the acid vapour having been seized, and the phlogiston left. And the result seems to determine this question in favour of the latter supposition. For when the process was over, I examined the water with the greatest attention, and found not the least appearance of acidity in

in it. It did not even turn the juice of turnsole red.

In the experiment above recited, concerning the diminution of nitrous air by iron filings and sulphur over a quantity of pure water, I first observed a pretty large deposit of a brownish kind of matter, from the water over which this process had been made. If this be really the *oxide of the iron* employed in it, it will shew that the earth of the metal had been volatilized by the heat of the fermentation.

That *liver of sulphur* also decomposes nitrous air by seizing upon its acid vapour seems to be proved by the following experiment. I put some pieces of liver of sulphur to two quantities of nitrous air confined by quicksilver, and I observed that, in about ten hours, between one third and one half of the air in each of them was absorbed. The next day I admitted water to one of them, but no part of the air was absorbed, and after it had passed several times through water, a candle burned in it with an enlarged flame, crackling very much. Also a candle burned in the very same manner in the air contained in the other vessel, which I had not made to pass through water.

The water over which the decomposition is made does not acquire the least acidity, not even discoverable by the juice of turnsole. In this case, also, there

there was an *earthy precipitate*, exactly as in the process with the iron filings and sulphur.

I had at one time some suspicion, that, as part of this kind of air is readily absorbed by water, exactly like fixed air, it might be, in part, real fixed air, and precipitate lime in lime water. But in the preceding experiments, as well as upon other occasions, I particularly attended to this circumstance, without ever observing it to have any such effect.

I endeavoured to decompose nitrous air by means of sulphur only; making use of a burning mirror and quicksilver for that purpose, which, however, is not an easy operation.

By this means, continuing the process a long time, I made the sulphur not only melt, but also become very black, and smoke very much in the nitrous air; and, in consequence of it, the air was considerably diminished, and had lost its power of diminishing common air in some degree. Had the operation been continued much longer, the diminution would, no doubt, have been more complete, and the nitrous air have become nothing more than phlogisticated air, as in other similar processes. That this diminution also was effected by means of the absorption of the nitrous vapour was pretty evident, because the mercury was not sensibly affected by it. For if that had been the case, there would ,

would have been an accession of more nitrous air from that solution.

I have mentioned a case, in which nitrous air, after having been exposed to iron, became not only partially inflammable, admitting a candle to burn in it with an enlarged flame, but was even fired with an explosion, like inflammable air from metals by oil of vitriol. I have since met with a more remarkable fact of the kind.

At the latter end of September 1778, I had put a pot of iron filings and sulphur into a jar of nitrous air, which, in the course of several days, was diminished by it in the usual proportion. From that time till the beginning of December, it had continued without any change that I had perceived; but about that time, imagining it was increased in bulk, I took exact notice of the dimensions of it, and presently found that the quantity was certainly increasing. Upon the whole, I concluded that it had increased about one sixth of its bulk, from the state of its greatest diminution. On the 11th of December I examined it, and found it to be proper inflammable air, being fired with many explosions when tried in the usual manner, but they were not so vigorous as those with fresh made inflammable air from iron and oil of vitriol.

After this, on the 12th of December, I put a pot of iron filings and sulphur to another quantity of

nitrous air, and on the 4th of February following it had increased in bulk about one third, and then burned with explosions like the former. But a quantity of nitrous air exposed to the effluvium of liver of sulphur, the very same time, never increased at all after the period of its utmost diminution and was mere phlogisticated air.

SECTION IV,

*Of dephlogisticated nitrous Air, procured by Iron, and
a Solution of Copper, in nitrous Acid.*

AFTER many trials, I succeeded in a method of producing this air, when I expected quite a different result. As iron will precipitate copper from a solution of it in spirit of nitre, and I had observed air to be generated in this process; I imagined that by putting iron into that solution of copper in spirit of nitre, I should get more
nitrous

nitrous air, and with less expence. But instead of this, I procured what I should much more have wished for, viz. this new species of nitrous air. But before I hit upon this, I succeeded in the method of changing fresh nitrous air into this species of it, in a remarkably short space of time.

Having at hand a phial filled with nails, which had often been employed in diminishing nitrous air, I filled it up with a diluted solution of copper in spirit of nitre, and left it all night. I then displaced the liquor by nitrous air, and in about two hours the whole quantity was diminished one half, and a candle burned in the remainder with an enlarged flame.

In this experiment I collected no air from the iron itself; but now, with the view that I have mentioned above, I filled the phial containing the nails with the solution of copper in spirit of nitre, and inverting it, the next morning I found the phial full of air, and it was not proper nitrous air, as I had expected, but that species of it in which a candle burns. In this case it burned quite naturally, and without any enlarged flame.

The quantity of this peculiar species of nitrous air that may be procured in this manner, and from the same materials, viz. without changing either the iron or the solution of copper, is astonishing. I made however, a pretty full trial of it, in a jar which
I had

I had thrust full of long pieces of iron wire, on purpose to expose as much of the surface of the iron as I possibly could, to any kind of air that I should afterwards put into it. This jar, which held about a pint and a half, I filled with the diluted solution of copper, and inverted it in a basin of the same solution; when, at the first, the jar was quite filled with this air in a few hours.

However, having got as much of this air as I wanted, I observed that more air was still generated; and inverting the jar every day, after I had filled it with the same solution of copper that had been expelled by the generated air the preceding day, it never failed to produce a jar full of that air every day for at least a fortnight, besides what escaped towards the beginning of the process from the mouth of the jar, as soon as it was full, and which I never collected.

I had imagined that, in time, the quality of this air would change, but to the very last it yielded air of the same kind in which a candle burned, not only naturally, but in a very vivid manner. If, however, I suffered the air to continue long in the jar, I always found it to be phlogisticated air; which, indeed, is the state to which this species of air is always reduced by long exposure in the same circumstances in which it is generated; the water either
absorbing

absorbing the dephlogisticated vapour, as it may be called, or this vapour getting saturated with phlogiston.

It was something remarkable, that a phial of nails which had often been used to diminish nitrous air, when filled with water only, yielded phlogisticated air. I at first filled this phial with inflammable air, and had observed a constant addition to it. It required, however, the warmth of a fire, when the phial was filled with water, to make it produce any considerable quantity of phlogisticated air. These nails had probably a quantity of nitrous vapour mixed with their rust, which might continue to act upon them; and this air, in its nascent state, might be nitrous, but afterwards become phlogisticated, according to the usual course of this process.

After I had written the above, I produced this kind of air much more readily than in the method there described, viz. by applying *heat* to the vessel in which it is produced. But I shall first observe that, immediately after producing a quantity of this air, in the manner before directed, I filled the vessel up with water instead of the solution of copper; and, in about two days, the vessel (which was a phial, containing near a quart) produced about three ounce measures of air, and it was phlogisticated air, extinguishing a candle.

Then

Then, pouring out the water, I filled it again with the solution of copper, and put into a pan of water, which I heated till it boiled; when, by means of a cork and bent glass tube, &c. I received from it about a quart of air, the first part of which was phlogisticated (owing perhaps to its having been in a state of yielding that air immediately before this process) but afterwards it was a proper dephlogisticated nitrous air, admitting a candle to burn in it quite naturally.

My original process for procuring dephlogisticated nitrous air requiring much time, I now always procure it by dissolving turnings of iron in a dilute solution of copper in the nitrous acid (the same that remains from making nitrous air) mixing with it again about an equal quantity of water. Without this precaution, though the iron will at first be acted upon very slowly, yet the mixture will at length grow so hot, as actually to boil, and the process will be exceedingly troublesome. It will be necessary, however, previously to heat the solution of copper, in order to expel from it all superfluous nitrous acid, though the keeping in it pieces of copper might answer the same purpose. Without one or other of these precautions, part of the air procured by dissolving iron in it will be proper nitrous air; but with this precaution, the whole of it will be such as
nitrous

nitrous air is reduced to by exposing common nitrous air to iron, that is, one third of it will generally be absorbed by water, and the remainder will be proper phlogisticated air.

SECTION V.

Of the Separation of the pure dephlogisticated nitrous Air from the phlogisticated Air with which it was mixed, and the Properties of it.

HAVING, in the manner mentioned above, procured a mixture of dephlogisticated nitrous air and phlogisticated air, the former will be rendered pure by agitating the mixture in water, which will imbibe it, and leave the phlogisticated part unabsorbed.

A given quantity of water, I find, will take up about one half of its bulk of this dephlogisticated nitrous air; and it does it almost as readily as water imbibes fixed air. The water thus saturated with

the air must be heated, and then the dephlogistified nitrous air will be expelled pure, and may be received in vessels containing mercury. I observed, however, that as this kind of air much resembles fixed air, in its properties of being imbibed by water, and in being expelled from it again by heat, it does so also in this farther property, that all the air which has been actually incorporated with the water, will not be imbibed by water again. But the proportion of this part is much greater in this kind of air than in fixed air. For in this case I have found it to be as one to ten; whereas in fixed air it is as about one to thirty or forty. This residuum being examined, was found to be air partially phlogistified, the standard of it being about 1.4, so that a candle will just burn in it; but the quality of this residuum is not always the same. The residuum of fixed air is generally much purer, viz. of the standard of 1.0.

I observed, that when water fully impregnated with this dephlogistified nitrous air is exposed to the atmosphere, it very soon parts with it, much sooner than water impregnated with fixed air.

I mixed pure dephlogistified nitrous air with all the other kinds of air that I have made, but it did not unite with any of them; and no sensible change was made in *it* by any of them, or by any of them in it; except that dephlogistified air was injured, being
I reduced

reduced from the standard of 0.3 to 0.8. But this was, no doubt, by the addition of that part of this air which is not miscible with water, and is considerably phlogisticated.

In this pure kind of dephlogisticated nitrous air a candle burned better than in common air, but not so well as I have frequently known it to do, as it is originally produced mixed with phlogisticated air; which seems to shew that some change is made in it by being incorporated with water. When it was mixed with inflammable air, they were exploded together with violence, very much like a mixture of inflammable and dephlogisticated air; but a mouse died in it as quickly as in fixed air.

That it is a mixture of pure nitrous air that is the cause of this thin blue flame with which the central flame in my former experiments is sometimes surrounded, is, I think, evident from the following experiment. Having a quantity of this air in which a candle burned with a strong and bright flame, I mixed nitrous air with it, and then the candle burned in it with the usual enlarged flame, a bluish flame surrounding the former.

In order to ascertain more exactly the degree of purity to which I could bring this air, I impregnated a quantity of snow water with it, and expelling it again by heat, I found that only one sixth of what had been expelled from the water, would not be imbibed by water again; so that, in this method,

it may be procured in a state of considerable purity.

Having a quantity of air in this state, and having found that a candle burned very well in it, I put a mouse into it; but it would have died very soon if I had not withdrawn it. This was on the 17th of March, 1781. But on the 21st of the same month, I put another mouse into the very same air, and was surprized to find that it continued perfectly at its ease five minutes. To be quite sure with respect to the other properties of this air, I withdrew the mouse while it was quite vigorous, and found that a candle burned very well in it, but it was not in the least affected by nitrous air. In this very singular case, nitrous air fails to be a test of the respirability of air. The air made use of in this experiment had been kept in a cup of mercury, but some water was in the vessel along with it, and this water had imbibed a little of the air. This, however, was the only case in which I have found a mouse to live in this kind of air, and therefore, though I cannot account for it, I do not lay much stress upon it.

I then expelled more of this air, from some water that had been impregnated with it on the 17th day of March, but a mouse died in it, and almost as soon as it would have done in any other kind of noxious air.

It was natural to suspect, that there might be something *acid* in this air. But though it was readily

dily absorbed by water tinged blue with the juice of turnsole, it made no change in its colour. Neither was there any thing alkaline in it. For when the slightest tinge of red was given to this liquor by an acid, the blue colour was never in the least degree restored by its impregnation with this kind of air.

The same thing may indeed be inferred from its not incorporating with either acid or alkaline air. In the experiment with the latter it appeared that this dephlogisticated nitrous air is lighter than common air. For mixing it with alkaline air, and then putting acid air to it, the white cloud formed by their union was almost wholly in the lower part of the vessel. But it is not so much lighter as inflammable air is found to be by the same experiment. For in this case the white cloud never reaches the top of the vessel; where the inflammable air appears to remain intirely unmixed with the alkaline air.

Thinking that, though dephlogisticated air ready formed might not affect, or be affected by, dephlogisticated nitrous air, it possibly might in its first formation; to try this, I revived mercury from red precipitate in this air. But the operation proceeded just as it would have done in vacuo, the mercury being revived very readily, and dephlogisticated air produced, without affecting, or being affected by, the dephlogisticated nitrous air.

No less remarkable was the result of heating malleable iron in this air. For the bulk of the air was increased, and yet afterwards no part of it was imbibed by water. It was all phlogisticated air, without any mixture of fixed air. It seems, therefore, as if this kind of air wanted nothing but phlogiston to make it phlogisticated air, and nothing but the principle of *heat*, to make it dephlogisticated air.

This experiment happening to draw my particular attention, I repeated it very often, and always had the same result, though with variations, depending on the quantity of the air, and the heat I was able to apply. If this was sufficient to melt the bits of iron, the effect was produced very quickly. As the particulars may be of use to future experimenters, I shall recite some of them.

Of three ounce measures and a half of dephlogisticated nitrous air, two ounce measures remained unabsorbed by water after heating malleable iron in it. During the process, the vessel had been full of whitish fumes. What remained of the air was of the standard of 1.7, and extinguished a candle.

Throwing the focus of the lens about five minutes on some filings of iron in this kind of air, the quantity was increased one tenth. Afterwards water absorbed about one sixth of it, and the remainder extinguished a candle, being of the standard of 1.8,

I repeated

I repeated the experiment with cast iron turnings, and had the same result. About one fifth was added to the quantity, water absorbed about the same proportion of it, and the remainder was of the standard of 1.85. Some little common air may be supposed to have been introduced into the vessel along with the turnings or shavings of iron, but this could not be much; and in all these experiments the addition produced in the process was a very small part of what remained unabsorbed by water; so that there can be no doubt but that this operation produces a real change in the quality of the air, rendering it almost wholly phlogisticated.

In order to produce this effect, the heat must be very considerable, or the pieces of iron very small, so that they must at least be red hot. For when I used whole cast iron nails, on which I suppose the heat of my lens could not make sufficient impression, the effect of applying it an hour and a half, and with a very good sun, was very inconsiderable; the proportion of that part of the air which was immiscible with water, being only increased in the proportion of fifty to twenty seven.

But the circumstance to which I happened to give the greatest attention with respect to this kind of air, was the change that is made in it by *heat*, or *light*. For such must be the agents, when bits of clean crucibles,

bles, or retorts, are heated by a burning lens in any kind of air. The consequence of this process always was both an increase of that part of the air which will not unite with water, and also rendering it purer than before, approaching nearer to the state of atmospherical air; exactly like the result of taking the electric spark in fixed air. I frequently stopped in the middle of this process, and always found these effects to be more considerable the longer I continued it. I shall give the particulars of those experiments only, on which I place the greatest dependence.

Having impregnated a quantity of boiled water with dephlogisticated nitrous air, I found that the air expelled from it was again absorbed by water, in the proportion of 0.7 to 10.1, and the standard of this residuum was 1.7. I then heated in it pieces of crucibles which had been just before made red hot, introducing them into the air by means of a forceps; and after heating them an hour and a half, in a very hot sun, found that a portion of the air was then unabsorbed by water, in the proportion of four to seven, and the standard of the residuum was 1.55. I resumed the experiment with what remained of the air, and continued it half an hour longer, when the proportion of the part that was not absorbed by water, was to the other as twenty six to forty, and the standard of this residuum was 1.45. No moisture
ever

ever appeared in the inside of the vessel, and there was no sensible change in the quantity of the air.

In a portion of the same dephlogisticated nitrous air that was used in the former experiment, I heated bits of crucibles, which, after being dipped in water, had only been dried, though they had been made very hot in the sun. In the course of this process, much water appeared in the vessel, expelled no doubt from the earthen ware; and after the process had been continued an hour and a half, the part of the air that was unabsorbed by water, was to the rest as 4.5 to 7, and the standard of it was 1.45.

Examining a quantity of dephlogisticated nitrous air, in a portion of which I was about to heat bits of dry crucibles over mercury, I found that by agitation in water 80.5 measures of it was reduced to 3.5 measures, and the standard of this residuum was 1.58. But of that portion of the same air in which bits of crucibles had been heated about an hour in all, sixty three measures were only reduced to twenty one, and the standard of this residuum was 1.38. On this occasion I observed that the inside of the glass vessel was pretty thickly coated with a black matter. Indeed, in the former experiments, it had always acquired a dark incrustation; but I did not know whether it might not have come from the earthen ware. I think it highly probable, that it was

was mercury super-phlogisticated, as every other black incrustation in processes in which mercury was used has been. It is probable, however, that some phlogisticated matter passed from the air to the mercury, which would of course render the residuum more phlogisticated, though I do not perceive how it should follow that it should thereby render it less miscible with water. But whatever proportion of phlogiston be necessary to make any kind of air miscible with water, any *change* in that proportion may make it less miscible with it.

In these respects this change in the constitution of dephlogisticated nitrous air, very much resembles that which is made in fixed air, both by the electric spark, and also by heating bits of crucibles in it; an account of which has been given before.

I endeavoured to ascertain whether this change in the constitution of dephlogisticated nitrous air was produced by mere *heat*, without light, by heating the air in earthen tubes or retorts, when their mouths were immersed in mercury; but, to my great disappointment, I found that all kinds of air heated in this manner, came out common air, or rather air a little phlogisticated. In fact, the earthen vessels, in that degree of heat to which I exposed them, must have been rendered pervious to air, though they were glazed inside and outside, and were perfectly air tight both before and after the experiment.

But

But the most effectual change that I was able to make in this kind of air, was bringing it to a state in which I doubt not it was respirable, by means of the electric spark. Taking this spark, or making small explosions, in 0.4 of an ounce measure of dephlogisticated nitrous air, it was rendered wholly immiscible with water, and of the standard of 1.45.

From this experiment it should seem, that there is in this species of air something that is capable of being converted into pure air. This, indeed, is sufficiently indicated both by a candle burning perfectly well in it, and also by pyrophorus spontaneously kindling in the nitrous air from which it is formed. A judicious prosecution of these experiments may therefore throw light upon the nature of both the kinds of air, and of combustion also.

This kind of air, however, though a candle burns so well in it, will not kindle pyrophorus. I put a small cupful of the pyrophorus into a quantity of this kind of air, and let it remain there a considerable time, without perceiving any sensible effect; but when I took it out, and put it into a vessel containing nitrous air, it took fire immediately.

In the course of these experiments with the sun, I observed a remarkable source of fallacy, with respect to the increase of the quantity of air confined by mercury, when there is so much moisture in the
inside

inside as to be subject to sudden dilations and compressions. For a considerable quantity of common air would get into the inside of the vessel, when there was the depth of an inch of mercury on the outside of it, and of two or three inches within. In these circumstances I have seen more than an ounce measure of the external air gain admission in less than one minute. This must have been occasioned by the mercury never being in perfect contact with glass; so that when the mercury was in a state of undulation, the air that was confined between it and the glass was continually protruded, and more air from the atmosphere was forced into its place, by the same pressure which supported the column of mercury within the glass. This effect I prevented by having a quantity of water upon the mercury, on the outside of the vessel. For this would be in perfect contact with the glass; and in this case I never found either air or water to get into the vessel to disturb my experiment.

I took the electric spark in a quantity of *dephlogisticated nitrous air*, till, without any perceivable change in its quantity, it was become immiscible with water. Then, admitting distilled water to it, there was no deposit made, though I could perceive a white incrustation quite round the inside of the glass vessel, contiguous to the mercury by which the air was confined. But admitting to it the water

in

in my trough, which happened to have mixed with it various solutions of iron, there was a copious white precipitate. This substance I had got once before, and found that by exposure to a red heat it became of a brown colour, and adhered firmly to the pieces of earthen ware on which it was heated, thereby appearing to be ochre. The whiteness may have been occasioned by the mercury.

SECTION VI.

Of Iron that has been used to diminish nitrous Air.

WHEN the iron nails or wires, which I have used to diminish nitrous air, had done their office, I laid them aside, not suspecting that they could be of any other philosophical use; but after having lain exposed to the open air almost a fortnight, having, for some other purpose, put some of them into a vessel containing common air, standing inverted, and immersed in water, I was surprized to observe that the air in which they were confined was diminished. The diminution proceeded

ceeded so fast, that the process was completed in about twenty four hours; for in that time the air was diminished about one fifth, so that it made no effervescence with nitrous air, and was therefore, no doubt, highly noxious, like air diminished by any other process.

This experiment I repeated a great number of times, with the same phials, filled with nails or wires that have been suffered to rust in nitrous air, but their power of diminishing common air grows less and less continually. How long it will be before it is quite exhausted I cannot tell. This diminution of air I conclude must arise from the phlogiston, with which the iron was overcharged by extracting it from the nitrous air, which it thereby left dephlogisticated. With this addition of phlogiston it would powerfully attract the dephlogisticated air in the atmosphere, and consequently leave the remainder phlogisticated.

Having a quantity of iron that was reduced to a perfect *rust* in these processes, I had the curiosity to heat it in an earthen crucible, in order to examine the *air* that might be expelled from it, and the result was as follows. From ten ounces of this rust, I got about 180 ounce measures of air, mixed at first with much steam. The first sixty ounce measures were chiefly fixed air, with a phlogisticated residuum, of the standard of 1.7. The remainder

mainder came much more slowly, requiring about four hours continuance of heat. This contained no mixture of fixed air, and was so pure, that a candle burned in it pretty well, being of the standard of 1.32. The matter was reduced to one mass, like iron, strongly attached by the magnet. But, indeed, the rust itself had been so before the process.

It seems probable, that the dephlogisticated nitrous vapour is contained in this rust, and that with the addition of the principle of heat it becomes proper dephlogisticated air. Or this dephlogisticated air might be that which by long exposure to the atmosphere it had extracted from it. It has been seen that that iron which has been exposed to nitrous air will diminish common air, and leave it phlogisticated.

In order to compare this kind of rust of iron with that which is formed by exposure to the atmosphere, I took four ounces of the latter; and, by the same treatment, got from it 180 ounce measures of air, about one half of which was fixed air, with a residuum of the standard of 1.7; but the bulk of it was inflammable, burning with a lambent blue flame.

To a quantity of this rust I put oil of vitriol with only two equal parts of water, and observed that it was but weakly acted upon, and that much heat was neces-

sary

fary to effect the solution of it. During the process much *black matter* was formed, and the inflammable air produced had an offensive smell. In these respects this iron seemed to approach to the nature of *cast iron*; but when it was melted by a burning lens in the open air, the parts of it were not dispersed like those of cast iron.

As yet I have seen no iron so far affected by this process, though it is in a state that easily crumbles to powder, as not to be able to decompose more nitrous air, and almost as readily as fresh iron.

I shall just mention one experiment in which this rusted iron, and fresh iron, were made use of. The nitrous air had been put to each of them on the 27th of August, and they were both examined on the 18th of October; when, in both cases, the air was found to be diminished to about one third, and a candle burned with equal brilliancy in both. Water then absorbed about one half of each of them, and the remainder extinguished a candle. Neither of them had in the smallest degree the property of fresh nitrous air.

SECTION VII.

Observations more particularly relating to the Constitution of dephlogisticated nitrous Air.

MY conjectures concerning the constitution of this kind of air will be found to have been various. But having discovered it at one time in a continued process, of the solution of zinc in spirit of nitre, always to come between the phlogisticated and dephlogisticated air, I supposed it to contain less phlogiston than phlogisticated air, and more than the dephlogisticated air; and therefore as it has some properties of a genuine dephlogisticated air, I am inclined, on the whole, to call it a *dephlogisticated nitrous air*.

Since a candle burns in this kind of air, and an animal dies in it, I think we are authorized to say, that it is of such a constitution, as to be capable of receiving phlogiston in a very great degree of heat, perhaps not short of a red heat, but not in that degree which is compatible with animal life. It is well known that many substances in chemistry can act upon one another, when they are hot, which do not at all affect one another when

they are cold. This, therefore, may be the case with this kind of air, and substances containing phlogiston.

That this species of air is really a dephlogisticated nitrous air, and not a phlogisticated air, as I had originally supposed, is more evident from its being produced from nitrous air by the *scales of iron*, which fly off when it is hammered, and which are iron partly reduced to a calx. I filled a phial with these scales, and then filling it up with mercury, dislodged the mercury with nitrous air. In this state the phial continued near three weeks; when I found the air diminished, I did not note how much, but a candle burned in the remainder just as if the nitrous air had been exposed to iron. These scales wanting much phlogiston to make them iron, must be in a state more ready to receive than to give phlogiston to nitrous air.

Water is absolutely necessary to this decomposition of nitrous air, when it is effected by iron. I had decomposed it before, either by previously filling the vessels that were to contain the nitrous air with *water*, or with *mercury*, though it had always required much longer time when mercury was used. The reason of this difference I did not then know; but I now perceive that it was owing to the small quantity of *moisture* in the vessel containing the mercury.

Suf-

Suspecting the influence of *water* in this process, I procured a number of very clean small needles, and having made a phial, and also a proper quantity of mercury, perfectly clean and dry, I put the needles into the phial; and filling it up with mercury, introduced the nitrous air. This phial continued in the same state six or eight months, without the least sensible change in the space occupied by the air. I therefore concluded that no change had taken place in it, and therefore introduced a few drops of water; after which I perceived, in a day or two, that the air was diminished, and the diminution proceeded till about one third of the air had disappeared. In this state the phial continued several months more; when, observing no farther change, I examined it, and found that a candle would not burn in it, which I had expected it would have done; as I imagined there was not water sufficient to imbibe that part of the air which had enabled a candle to burn in it; and how this part of the air should disappear, or be converted into phlogisticated air, without water, I could not tell. In another experiment the result was seemingly different.

On the 26th of May, 1782, I examined a quantity of nitrous air, which had been confined by mercury with iron shavings from the 27th of August preceding, when I found one half of it absorbed,

and a candle burned in the remainder better than in common air, though a mouse died in it; and yet this air had remained several months in the same state, with respect to quantity, in which it was when I examined it; so that I concluded it would never have extinguished a candle, in consequence of any longer continuance in that state. In this case, however, there was no sensible quantity of water at all; whereas in the other there were a few drops. Water, therefore, may contribute to this change in the air, independently of its *absorbing* any part of it. When I agitated this air in water, something more than one half was absorbed, and the remainder extinguished a candle.

But, perhaps, the more probable inference from this experiment may be that, in time, all the dephlogisticated nitrous air is imbibed by the iron, so that nothing besides the phlogisticated air will be left in the vessel; though there was no appearance of this being the case in the experiment with the needles; as the air did not continue to diminish longer than usual, and there remained two thirds of the original quantity of phlogisticated air. This, therefore, rather looks like a change of the dephlogisticated nitrous air into phlogisticated air, by the addition of phlogiston from the iron.

Another experiment I made with a view to ascertain the degree of diminution at which the nitrous
air

air begins to be decomposed. On the 14th of May, I put twenty four ounce measures of nitrous air into a quart bottle, previously filled with pieces of iron, that had before been rusted in this process. On the 18th there were only seventeen ounce measures of air, and it was not at all changed; on the 21st there were thirteen ounce measures, and on the 31st four ounce measures, of which half was then absorbed by water, and the remainder was phlogisticated air. This was the greatest diminution that I remember to have observed in this process. But having neglected it from the 21st to the 31st, I did not find the time at which the decomposition first took place; but in general I believe it is when about one fourth of the quantity is remaining, and that the change takes place pretty suddenly, for I do not remember ever to have found it in a middle state.

B O O K IV.

OF DEPHLOGISTICATED AIR.

P A R T I.

OF THE SOURCES OF DEPHLOGISTICATED AIR.

S E C T I O N I.

*An Account of the Discovery of dephlogisticated Air,
and its general Properties.*

THE contents of this section will furnish a very striking illustration of the truth of a remark, which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking,

ing, to the observation of *events arising from unknown causes*, than to any proper *design*, or preconceived *theory* in this business. This does not appear in the works of those who write *synthetically* upon these subjects; but would, I doubt not, appear very strikingly in those who are the most celebrated for their philosophical acumen, did they write *analytically* and ingenuously..

For my own part, I will frankly acknowledge, that, at the commencement of the experiments recited in this section, I was so far from having formed any hypothesis that led to the discoveries I made in pursuing them, that they would have appeared very improbable to me had I been told of them; and when the decisive facts did at length obtrude themselves upon my notice, it was very slowly, and with great hesitation, that I yielded to the evidence of my senses. And yet, when I re-consider the matter, and compare my last discoveries relating to the constitution of the atmosphere with the first, I see the closest and the easiest connexion between them, so as to wonder that I should not have been led immediately from the one to the other. That this was not the case, I attribute to the force of prejudice, which, unknown to ourselves, biases not only our *judgments*, properly so called, but even the perceptions of our senses; for we may take a maxim so strongly for granted, that

the plainest evidence of sense will not entirely change, and often hardly modify, our persuasions; and the more ingenious a man is, the more effectually he is entangled in his errors; his ingenuity only helping him to deceive himself, by evading the force of truth.

There are, I believe, very few maxims in philosophy that have laid firmer hold upon the mind, than that air, meaning atmospherical air (free from various foreign matters, which were always supposed to be dissolved, and intermixed with it) is *a simple elementary substance*, indestructible, and unalterable, at least as much so as water is supposed to be. In the course of my inquiries, I was, however, soon satisfied that atmospherical air is not an unalterable thing; for that, according to my first hypothesis, the phlogiston with which it becomes loaded from bodies burning in it, and animals breathing it, and various other chemical processes, so far alters and depraves it, as to render it altogether unfit for inflammation, respiration, and other purposes to which it is subservient; and I had discovered that agitation in water, the process of vegetation, and probably other natural processes, restore it to its original purity. But I own I had no idea of the possibility of going any farther in this way, and thereby procuring air purer than the best common air. I might, indeed, have naturally imagined that such would be air that should contain
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less phlogiston than the air of the atmosphere; but I had no idea that such a composition was possible.

In my first publication on the subject of air, I mentioned that which I had got from *nitre*, and the account I then gave of it demonstrates it to have been dephlogisticated air, but I had not pursued that experiment, nor was it of any use to me in the following course. It may be worth while, however, to recite what I then observed, which was as follows.

All the kinds of factitious air, I then observed, on which I had made the experiment, were highly noxious, except that which is extracted from *nitre*, or *alum*; but in this even a candle burned just as in common air. In one quantity which I got from *nitre* a candle not only burned, but the flame was increased, and something was heard like a hissing, similar to the decrepitation of *nitre* in an open fire.

The air was extracted from these substances by heating them in a gun barrel, which was much corroded and soon spoiled by the experiment. What effect this circumstance had upon the air I did not consider.

At the time of my first publication, I was not possessed of a *burning lens* of any considerable force; and for want of one, I could not possibly make many of the experiments that I had projected, and
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which, in theory, appeared very promising. I had, indeed, a *mirror* of force sufficient for my purpose. But the nature of this instrument is such, that it cannot be applied, with effect, except upon substances that are capable of being suspended, or resting on a very slender support. It cannot be directed at all upon any substance in the form of *powder*, nor hardly upon any thing that requires to be put into a vessel of quicksilver; which appears to me to be the most accurate method of extracting air from a great variety of substances, as was explained in the introduction to this work. But having afterwards procured a lens of twelve inches diameter, and twenty inches focal distance, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances, natural and factitious, would yield, putting them into the vessels represented fig. *a*, Pl. IV. which I filled with quicksilver, and kept inverted in a basin of the same. Mr. Warltire, a good chemist, and lecturer in natural philosophy, happening to be at that time in Calne, I explained my views to him, and was furnished by him with many substances, which I could not otherwise have procured.

With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1st of August, 1774, I endeavoured to extract air from *mercurius calcinatus per*

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se; and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprized me more than I can well express, was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, exposed to iron or liver of sulphur; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous acid was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it.

In this case, also, though I did not give sufficient attention to the circumstance at that time, the flame of the candle, besides being larger, burned with more splendor and heat than in that species of nitrous air; and a piece of red hot wood sparkled in it, exactly like paper dipped in a solution of nitre, and it consumed very fast; an experiment which I had never thought of trying with dephlogisticated nitrous air.

At the same time that I made the above-mentioned experiment, I extracted a quantity of air, with the very same property, from the common *red precipitate*, which being produced by a solution of mercury in spirit of nitre, made me conclude that this

peculiar property, being similar to that of the modification of nitrous air above mentioned, depended upon something being communicated to it by the nitrous acid; and since the *mercurius calcinatus* is produced by exposing mercury to a certain degree of heat, where common air has access to it, I likewise concluded that this substance had collected something of *nitre*, in that state of heat, from the atmosphere.

This, however, appearing to me much more extraordinary than it ought to have done, I entertained some suspicion that the *mercurius calcinatus*, on which I had made my experiments, being bought at a common apothecary's, might, in fact, be nothing more than red precipitate; though, had I been any thing of a practical chymist, I could not have entertained any such suspicion. However, mentioning this suspicion to Mr. Warltire, he furnished me with some that he had kept for a specimen of the preparation, and which, he told me, he could warrant to be genuine. This being treated in the same manner as the former, only by a longer continuance of heat, I extracted much more air from it than from the other.

This experiment might have satisfied any moderate sceptic; but, however, being at Paris in the October following, and knowing that there were several very eminent chymists in that place, I did not

not omit the opportunity, by means of my friend Mr. Magellan, to get an ounce of *mercurius calcinatus* prepared by Mr. Cadet, of the genuineness of which there could not possibly be any suspicion; and at the same time, I frequently mentioned my surprise at the kind of air which I had got from this preparation to Mr. Lavoisier, Mr. le Roy, and several other philosophers, who honoured me with their notice in that city; and who, I dare say, cannot fail to recollect the circumstance.

At the same time, I had no suspicion that the air which I had got from the *mercurius calcinatus* was even wholesome; so far was I from knowing what it was that I had really found; taking it for granted, that it was nothing more than such kind of air as I had brought nitrous air to be by the processes above mentioned; and in this air I have observed that a candle would burn sometimes quite naturally, and sometimes with a beautiful enlarged flame, and yet remain perfectly noxious.

At the same time that I had got the air above mentioned from *mercurius calcinatus* and the red precipitate, I had got the same kind from red lead or *minium*. In this process, that part of the minium on which the focus of the lens had fallen, turned yellow. One third of the air, in this experiment, was readily absorbed by water, but, in the remainder,

der, a candle burned very strongly, and with a crackling noise.

This experiment with *red lead* confirmed me more in my suspicion, that the *mercurius calcinatus* must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation, and this of red lead is made, being similar. As I never make the least secret of any thing that I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, and the red precipitate, to all my philosophical acquaintance at Paris, and elsewhere; having no idea at that time, to what these remarkable facts would lead.

Presently after my return from abroad, I went to work upon the *mercurius calcinatus*, which I had procured from Mr. Cadet; and, with a very moderate degree of heat, I got from some of it, an ounce measure of air, which I observed to be not readily imbibed, either by the substance itself from which it had been expelled (for I suffered them to continue a long time together before I transferred the air to any other place) or by water, in which I suffered this air to stand a considerable time before I made any experiment upon it.

In this air, as I had expected, a candle burned with a vivid flame; but what I observed new at this time (Nov. 19) and which surprized me no less

less than the fact I had discovered before, was, that, whereas a few moments agitation in water will deprive the modified nitrous air of its property of admitting a candle to burn in it; yet, after more than ten times as much agitation as would be sufficient to produce this alteration in the nitrous air, no sensible change was produced in this. A candle still burned in it with a strong flame; and it did not, in the least, diminish common air, which I had observed that nitrous air, in this state, in some measure, does.

But I was much more surprized, when, after two days, in which this air had continued in contact with water (by which it was diminished about one twentieth of its bulk) I agitated it violently in water about five minutes, and found that a candle still burned in it as well as in common air. The same degree of agitation would have made phlogisticated nitrous air fit for respiration indeed, but it would certainly have extinguished a candle.

These facts fully convinced me, that there must be a very material difference between the constitution of the air from *mercurius calcinatus*, and that of dephlogisticated nitrous air, notwithstanding their resemblance in some particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for respiration, after being agitated in water, as every kind of air without exception, on which I had
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tried the experiment, had been, I still did not suspect that it was respirable in the first instance; so far was I from having any idea of this air being, what it really was, much superior, in this respect, to the air of the atmosphere.

In this ignorance of the real nature of this kind of air, I continued from this time (November) to the 1st of March following; having, in the mean time, been intent upon my experiments on the vitriolic acid air, and the various modifications of air produced by spirit of nitre. But in the course of this month, I not only ascertained the nature of this kind of air, though very gradually, but was led by it, as I then thought, to the complete discovery of the constitution of the air we breathe.

Till this first of March, 1775, I had so little suspicion of the air from *mercurius calcinatus*, &c. being wholesome, that I had not even thought of applying to it the test of nitrous air; but thinking (as my reader must imagine I frequently must have done) on the candle burning in it after long agitation in water, it occurred to me at last to make the experiment; and putting one measure of nitrous air to two measures of this air, I found, not only that it was diminished, but that it was diminished quite as much as common air, and that the redness of the mixture was likewise equal to that of a similar mixture of nitrous and common air.

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After this I had no doubt but that the air from *mercurius calcinatus* was fit for respiration, and that it had all the other properties of genuine common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater than common air would have admitted.

I now concluded that all the constituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this substance, and also in the process of making red lead. For at the same time that I made the above mentioned experiment on the air from *mercurius calcinatus*, I likewise observed that the air which I had extracted from red lead, after the fixed air was washed out of it, was of the same nature, being diminished by nitrous air like common air; but, at the same time, I was puzzled to find that air from the red precipitate was diminished in the same manner, though the process for making this substance is quite different from that of making the two others. But to this circumstance I happened not to give much attention.

I wish my reader be not quite tired with the frequent repetition of the word *surprize*, and others of similar import; but I must go on in that style a little

tle longer. For the next day I was more surprized than ever I had been before, with finding that, after the above mentioned mixture of nitrous air and the air from *mercurius calcinatus*, had stood all night (in which time the whole diminution must have taken place; and, consequently, had it been common air, it must have been made perfectly noxious, and intirely unfit for respiration or inflammation) a candle burned in it, and even better than in common air.

I cannot, at this distance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real issue of it. Having acquired a considerable degree of readiness in making experiments of this kind, a very slight and evanescent motive would be sufficient to induce me to do it. If, however, I had not happened, for some other purpose, to have had a lighted candle before me, I should probably never have made the trial; and the whole train of my future experiments relating to this kind of air might have been prevented.

Still, however, having no conception of the real cause of this phenomenon, I considered it as something very extraordinary; but as a property that was peculiar to air extracted from these substances, and *adventitious*; and I always spoke of the air to my acquaintance as being substantially the same thing

thing with common air. I particularly remember my telling Dr. Price, that I was myself perfectly satisfied of its being common air, as it appeared to be so by the test of nitrous air; though, for the satisfaction of others, I wanted a mouse to make the proof quite complete.

On the 8th of this month I procured a mouse, and put it into a glass vessel, containing two ounce measures of the air from *mercurius calcinatus*. Had it been common air, a full grown mouse, as this was, would have lived in it about a quarter of an hour. In this air, however, my mouse lived a full half hour; and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled; for, upon being held to the fire, it presently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my conclusion, that the air extracted from *mercurius calcinatus*, &c. was *at least as good* as common air; but I did not certainly conclude that it was any *better*; because, though one mouse would live only a quarter of an hour in a given quantity of air, I knew it was not impossible but that another mouse might have lived in it half an hour; so little accuracy is there in this method of ascertaining the goodness of air: and indeed I have never had recourse to it for my own satisfaction, since the discovery of that most ready,

accurate, and elegant test that nitrous air furnishes. But in this case I had a view to publishing the most generally-satisfactory account of my experiments that the nature of the thing would admit of.

This experiment with the mouse, when I had reflected upon it some time, gave me so much suspicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the test of nitrous air to a small part of that very quantity of air which the mouse had breathed so long; so that, had it been common air, I was satisfied it must have been very nearly, if not altogether, as noxious as possible, so as not to be affected by nitrous air; when, to my surprize again, I found that though it had been breathed so long, it was still better than common air. For after mixing it with nitrous air, in the usual proportion of two to one, it was diminished in the proportion of four and a half to three and a half; that is, the nitrous air had made it two ninths less than before, and this in a very short space of time; whereas I had never found that, in the longest time, any common air was reduced more than one fifth of its bulk by any proportion of nitrous air, nor more than one fourth by any phlogistic process whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another measure of nitrous air to the same mixture, and, to my utter astonishment, found that
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it was farther diminished to almost one half of its original quantity. I then put a third measure to it; but this did not diminish it any farther; but, however, left it one measure less than it was even after the mouse had been taken out of it.

Being now fully satisfied that this air, even after the mouse had breathed it half an hour, was much better than common air; and having a quantity of it still left, sufficient for the experiment, viz. an ounce measure and a half, I put the mouse into it; when I observed that it seemed to feel no shock upon being put into it, evident signs of which would have been visible, if the air had not been very wholesome; but that it remained perfectly at its ease another full half hour, when I took it out quite lively and vigorous. Measuring the air the next day, I found it to be reduced from one and a half to two thirds of an ounce measure. And after this, if I remember well (for in my *register* of the day I only find it noted, that it was *considerably diminished* by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouse having been taken out quite vigorous, that the air could not have been rendered very noxious.

For my farther satisfaction I procured another mouse, and putting it into less than two ounce measures of air extracted from *mercurius calcinatus* and

air from red precipitate (which, having found them to be of the same quality, I had mixed together) it lived three quarters of an hour. But not having had the precaution to set the vessel in a warm place, I suspect that the mouse died of cold. However, as it had lived three times as long as it could probably have lived in the same quantity of common air, and I did not expect much accuracy from this kind of test, I did not think it necessary to make any more experiments with mice.

Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity, with as much accuracy as I could, by the test of nitrous air; and I began with putting one measure of nitrous air to two measures of this air; as if I had been examining common air; and now I observed that the diminution was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two thirds of its original quantity, and a third measure to one half. Suspecting that the diminution could not proceed much farther, I then added only half a measure of nitrous air. By this it was diminished still more, but not much, and another half measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures of nitrous

trous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.

At the same time, air from the *red precipitate* was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half measures before it ceased to be diminished by more nitrous air, and even five half measures made no addition to its original dimensions, I concluded that it was between four and five times as good as common air. It will be seen that I have since procured air purer than this.

SECTION II.

Of the Production of dephlogisticated Air, by Means of Spirit of Nitre and the Calx of Lead.

BEING now fully satisfied with respect to the nature of this new species of air, viz. that, being capable of taking more phlogiston from nitrous air, it therefore originally contains less of this principle; my next inquiry was, by what means it comes to be so pure, or philosophically speaking, to be so much *dephlogisticated*; and since the red lead yields the same kind of air with *mercurius calcinatus*, though mixed with fixed air, and is a much cheaper material, I proceeded to examine all the preparations of lead, made by heat in the open air, to see what kind of air they would yield, beginning with the *grey calx*, and ending with *litharge*.

The red lead which I used for this purpose yielded a considerable quantity of dephlogisticated air, and very little fixed air. In this experiment two measures of nitrous air being put to one measure of this air, reduced it to one third of what it was at first,

first, and nearly three times its bulk of nitrous air made very little addition to its original dimensions ; so that this air was exceedingly pure, and better than any that I had procured before.

One quantity of red lead, which I procured, had a yellowish cast, and had in it several pieces intirely yellow. I tried it with the burning lens in quick-silver, and found that it yielded very little air, and with great difficulty ; requiring the application of a very intense heat. With an equal quantity of nitrous air, a part of this air was reduced to one half of its original bulk, and three measures and a half saturated it. The air, therefore, was very pure, and the quantity that it yielded being very small, it proved to be in a very favourable state for ascertaining on what circumstances its acquiring this air depended.

My object now was to bring this red lead, which yielded very little air, to that state in which other red lead had yielded a considerable quantity ; and taking it, in a manner, for granted, in consequence of the reasoning intimated above, that red lead must imbibe from the atmosphere some kind of *acid*, in order to acquire that property, I took three separate half ounces of this fresh made red lead, and moistened them till they made a kind of paste, with each of the three mineral acids, viz. the vitriolic, the marine, and the nitrous ; and as I intended to make the

the experiment in a gun barrel, lest the iron should be too much affected by them, I dried all these mixtures, till they were perfectly hard; then pulverizing them, I put them separately into my gun barrel, filled up to the mouth with pounded flint, which I had found by trial to yield little, or no air when treated in this manner. I had also found that no quantity of air, sufficient to make an experiment, could be procured from an equal quantity of this red lead by this process.

Those portions of the red lead which had been moistened with the vitriolic and marine acids became white; but that which had been moistened with the nitrous acid, had acquired a deep brown colour. The mixtures with the nitrous and marine acids dried pretty readily, but that with the vitriolic acid was never perfectly dry; but a great part of it remained in the form of a softish paste.

Neither the vitriolic nor the marine acid mixtures gave the least air when treated in the manner above-mentioned; but the moment that the composition into which the *nitrous* acid had entered became warm, air began to be produced; and I received the produce in quicksilver. About one ounce measure was quite transparent, but presently after it became exceedingly red; and being satisfied that this redness was owing to the nitrous acid vapour having dissolved the quicksilver, I took no

more than two ounce measures in this way, but received all the remainder, which was almost two pints, in water. Far the greatest part of this was fixed air, being readily absorbed by water, and extinguishing a candle. There was, however, a considerable residuum, in which the flame of a candle burned with a crackling noise, from which I concluded that it was true dephlogisticated air.

In this experiment I had moistened the red lead with spirit of nitre several times, and had dried it again. When I repeated the experiment, I moistened it only once with the same acid, when I got from it not quite a pint of air; but it was almost all of the dephlogisticated kind, about five times as pure as common air. N. B. All the acids made a violent effervescence with the red lead.

Though there was a difference in the result of these experiments, I was now convinced that it was the nitrous acid which the red lead had acquired from the air, and which had enabled it to yield the dephlogisticated air, agreeably to my original conjecture. Finding also, as will be seen in the following section, that the same kind of air is produced by moistening with the spirit of nitre any kind of earth that is free from phlogiston, and treating it as I had done the red lead in the last-mentioned experiment, there remained at that time no doubt in my mind, but that *atmospherical air*,
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or the thing that we breathe, *consists of the nitrous acid and earth*, with so much phlogiston as is necessary to its elasticity; and likewise so much more as is required to bring it from its state of perfect purity to the mean condition in which we find it.

From the following experiments it will appear that the quantity of dephlogisticated air depends upon the quantity of the spirit of nitre made use of in the process, the quantity of fixed air being nearly the same in all the cases.

From an ounce of red lead, heated in a gun barrel, I got about an ounce measure of air, which all together was worse than common air; an effect which I attribute, in a great measure, to phlogiston discharged from the iron. The production of air in this case was very slow.

From an ounce measure of the same red lead, diluted with half spirit of nitre and half water, I got twelve ounce measures of air, the last produce of which was highly nitrous. Half of this quantity was absorbed by water, and the remainder was twice as good as common air.

From an ounce of the same red lead, diluted with the same spirit of nitre, without water, I got, by the same treatment, about thirty ounce measures of air, about one eighth of which was absorbed by water, while the rest was highly dephlogisticated.

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From the same quantity of red lead, moistened with twice the quantity of the same spirit of nitre, I got about sixty ounce measures of air, a very small part of which was absorbed by water, and the rest was as highly dephlogisticated as that in the last experiment.

The produce of air was quicker, with the same degree of heat, in proportion as the quantity produced was greater; and in the last process the air was very red in the inside of the vessel that received it, for a considerable time.

For this purpose I tried, with success, *flowers of zinc, chalk, quick lime, slacked lime, tobacco pipe clay, flint* and *Muscovy talck*, with other similar substances, which will be found to comprize almost all the kinds of earth that are essentially distinct from each other, according to their chemical properties.

I was the more confirmed in my idea of spirit of nitre and earth constituting respirable air, by finding, that when any of these matters, on which I had tried the experiment, had been treated in the manner above-mentioned, and they had thereby yielded all the air that could be extracted from them by this process; yet when they had been moistened with fresh spirit of nitre, and were treated in the same manner as before, they would yield as much dephlogisticated air as at the first. This may

may be repeated till all the earthy matter be exhausted. It will be sufficient to recite one or two facts of this kind from my register.

April 18, I took the remains of the fresh made red lead, out of which a great quantity of dephlogisticated air had been extracted, and moistening about three quarters of an ounce of it a second time with spirit of nitre, I got from it about two pints of air, all of which was nearly six times as pure as common air. This air was generated very fast, and the glass tube through which it was transmitted was filled with red fumes; the nitrous acid, I suppose, prevailing in the composition of the air, but being absorbed by the water in which it was afterwards received.

In this, and many other processes, my reader will find a great variety in the purity of the air procured from the same substances. But this will not be wondered at, if it be considered that a small quantity of phlogistic matter, accidentally mixing with the ingredients for the composition of this air, depraves it. It will also be unavoidably depraved, in some measure, if the experiment be made in a gun barrel, which I commonly made use of, when, as was generally the case, it was sufficiently exact for my purpose, on account of its being the easiest, and in many respects, the most commodious instrument.

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The reason of this is, that if the produce of air be not very rapid, there will be time for the phlogiston to be disengaged from the iron itself, and to mix with the air. Accordingly I have seldom failed to find, that when I endeavoured to get all the air I possibly could from any quantity of materials, and received the produce at different times (as for my satisfaction I generally did) the last was inferior in purity to that which came first. Not unfrequently it was phlogisticated air; that is, air so charged with phlogiston, as to be perfectly noxious. If, therefore, any person shall propose to make dephlogisticated air, in large quantities, he should have an apparatus appropriated to that purpose; and the greatest care should be taken to keep the instruments as clear as possible from all phlogistic matter, which is the very bane of purity with respect to air, they being exactly *plus* and *minus* to each other.

In these experiments I was led into an error by not giving sufficient heat to the mixture of red lead and vitriolic acid, or rather by using a gun barrel. Had I been then in possession of the excellent *earthen tubes* with which Mr. Wedgwood now furnishes me, I should have got as pure dephlogisticated air from the vitriolic, as the nitrous acid.

SECTION III.

A more particular Account of some Processes for the Production of dephlogisticated Air, in order to determine what Kind of Earth was most proper to mix with nitrous Acid for the Purpose.

HAVING seen sufficient reason, as I at first thought, to conclude that respirable air consists of nitrous acid and earth, my object, in all this course of experiments, was simply to find *what kind of earth* was most proper for this purpose, or which had the most aptness to form this peculiar union with the nitrous acid. Upon the whole, I think it will appear that the *metallic earths*, if they be free from phlogiston, are the most proper, and next to them the *calcareous earths*; but that a very great difference in the production of this kind of air depends upon a variety of circumstances in which the experiments are made.

The purest air that I at that time procured was from *flowers of zinc*, moistened, as in the other processes, with spirit of nitre, and put into a glass phial, with a ground-stopper and tube. At first I despaired of getting any air at all from the process;

cess; but at length it came in a prodigious torrent, and was so cloudy, that the bursting of every bubble, after it had passed through the water, resembled the bursting of a bag of flour. The tube through which it was transmitted was exceedingly red, and in some degree, the inside of the receiver too, as might be perceived amidst the thick cloud that filled it. This cloudiness of the newly-generated air, I have often perceived in the process with red lead, but never in so great a degree as in this case.

The quantity of air procured was nearly three pints, from about half an ounce measure of the flowers of zinc; and it was so highly dephlogisticated, that it took three times its bulk of nitrous air before its dimensions were increased. When it had got only twice its bulk of nitrous air, it was reduced to less than one fifth of its original quantity. The last produce came very slowly, and was not quite so pure. The flowers of zinc, which I used in this experiment, formed a very hard and brittle substance, when mixed with spirit of nitre, and dried. After the process it swelled, and broke the phial into many pieces.

Next to the metallic earths of lead and zinc, I found the *calcareous* earths the most proper for the production of dephlogisticated air; but I had no opportunity of trying any great variety of them.

The best that I did try was *chalk*. Having saturated half an ounce of it with diluted spirit of nitre, and dried it, I got from it, in a gun barrel, more than a pint of air, which was highly dephlogisticated. I began to receive this produce in quicksilver, the consequence of which was, that the nitrous acid, coming over in the form of vapour, dissolved the quicksilver, and made nitrous air; but a crust being formed upon the surface of it, prevented the solution of more, and the air continued red a long time.

From another ounce measure of chalk, treated in the same manner, I got about a quart of air. What I took first was considerably nitrous, two measures of common air and one of this, occupying the space of two measures and a half. The second pint was dephlogisticated; so that two measures of it, and five of nitrous air, occupied the space of two measures. The last was less dephlogisticated, being about one half better than common air. At this time the air was generated with prodigious rapidity; the glass tube through which it was transmitted was exceedingly red; and when, in changing the vessels, some of the vapour escaped into the air, it had the reddest appearance of any thing that I had ever seen of the kind.

Having saturated half an ounce of exceedingly good *quick-lime* with diluted spirit of nitre, dried it,

it, and put it into a gun barrel; I got from it about a pint and half of air, the first part of which was so far dephlogisticated, that it required an equal measure of nitrous air to saturate it. The second was no better than common air, and the third was equal to the first. In this process the air was produced very irregularly, sometimes coming in great quantities, and at other times the water would rush back into the tube.

I repeated the experiment with quick-lime, in a glass phial and tube, when the whole quantity was so pure, that it required twice its bulk of nitrous air to saturate it. The produce of air, in this experiment, was as irregular as in the preceding.

It is sufficiently evident from these experiments, that dephlogisticated air is produced by means of all kinds of earth mixed with spirit of nitre; only that a greater quantity of air is produced from some than from others; the advantage in this respect being on the side of the metallic and calcareous earths. The reason, though I was not then aware of it is, that those earths, by their affinity to the nitrous acid, retain it till it can be acted upon by the fire. For in all cases it is the *acid* only, and not the *earth*, from which the air comes.

I would observe, that this process seems to furnish a pretty accurate test, perhaps the most accurate hitherto known, of the presence of phlogiston in

bodies. Nitrous air contains so much phlogiston, that I think it cannot be produced unless the materials themselves contain it in a very considerable degree. Whereas, if the air be highly dephlogisticated, I think it may be considered as the most satisfactory proof we are yet acquainted with, that the substance contains no phlogiston at all.

I had at this time made trial of no more than two of the metallic earths, viz. those of *lead* and *zinc*. The reason why I did not proceed any farther in this way was, that I knew not how to procure the earths of the several metals in a state tolerably free from phlogiston, without which I was well aware they would have yielded nitrous or phlogisticated air, and not dephlogisticated.

But I afterwards hit upon a method of coming at the pure earth of all the metals, with the least trouble possible, also of determining the quantity of phlogiston they each contain, with the quantity of pure earth which remains after all the phlogiston is expelled, and the quality of it, with respect to what I then thought its *convertibility into air*. This is effected by dissolving the metals completely, and distilling the solution to dryness in a glass vessel, then employing more spirit of nitre, till as much as I please of it be converted into air. If much phlogiston do not adhere to the calx of the metal, pure air will be procured at the very first distillation; but if the earth be very impure,

pure, by containing much phlogiston, the whole of the first produce will be nitrous, or phlogisticated air, and pure air may not be procured till the second or third process. It was from *copper* that I first procured air in this manner, as will be related presently when I come to treat of air from that metal. But I chuse, for the sake of better order, to begin with my accounts of the more *perfect metals*, before I mention the more imperfect ones, and I shall relate all the appearances, though some of them are not to the present purpose.

I dissolved a quantity of *gold* in *aqua regia*, and found that it had lost four grains in weight. During this solution it yielded an ounce measure of air, mixed with a good deal of acid vapour; for when lime water was admitted to it, though it did not become in the least degree turbid, it absorbed more than half of the air. After this I distilled the solution to dryness, and got from it about half an ounce measure of air, half of which, judging by the same appearances, was acid vapour, and the remainder pure dephlogisticated air. A brownish powder remained, which, being collected as carefully as I could, weighed three grains; so that the air above-mentioned had been produced from one grain of gold only, and probably something less. The powder that remained was pure gold, being soluble in *aqua regia*, as I found some months afterwards,

This solution of gold was made in the common *aqua regia*, consisting of one fourth of marine acid and three fourths of spirit of nitre; and it was remarkable that when I made another solution of gold in that kind of *aqua regia* which I made by impregnating marine acid with nitrous vapour, (which is a much more powerful menstruum for gold than the common *aqua regia*, yet containing less spirit of nitre) so great a quantity of the air procured from the solution was not dephlogisticated. The particulars of this process were as follows.

I dissolved a pennyweight of gold in this new, but more powerful kind of *aqua regia*; and distilling the solution to dryness, in a glass phial, with a ground stopper, I got from it about eight ounce measures of air, which did not make lime water turbid, but was so much absorbed by water, that not more than one fourth of an ounce measure was left; and this was not better than common air, but it might be a little depraved by some part of the acid vapour still remaining in it. When this process approached to dryness, the recipient was filled with an exceedingly thick and white cloud. In this experiment also the residuum was intirely dissolved by fresh *aqua regia*.

In some of these experiments I imagined that all the residuum was not wholly dissolved by *aqua*

regia, but the quantity undissolved was exceedingly small, and might be some impurity in the gold that I made use of. However, that a real *calx of gold* was produced in these experiments, is, I think, probable, from the *tinge* given to the glass in which the process was made, which, when the heat was considerable, was partly purple, and partly a dark gold colour, which no acid would touch. But I must observe, that a very slight tinge of the same colour I once observed to be given to a glass vessel of the same kind, in which *nitre* only had been melted. I am not, therefore, absolutely sure that, in the former case, the tinge was given by the gold, though I think it by much the most probable supposition.

From four pennyweights six grains of *silver* dissolved in the nitrous acid, diluted with one third part of water, I got five ounce measures of nitrous air; and removing the vessel to a sand heat, I got a little very pure air, without any mixture of fixed air; but the vessel breaking, I cannot tell how much more might have been procured.

I poured diluted spirit of nitre upon *quicksilver*, till I had got about three ounce measures of nitrous air without the application of heat; when pouring out the undissolved quicksilver, I put the solution, together with the crystals (a third part of the vessel being filled with them) into a sand heat; when I

got first forty ounce measures of nitrous air, and immediately after that about the same quantity of the purest dephlogisticated air. During the rapid production of this air, it was exceedingly white, and the tube through which it was conveyed, was very red with the redundant nitrous vapour. Towards the end of the process the tube was transparent, and colourless, though the air burst in white clouds within the recipient.

The first time that I procured dephlogisticated air from *copper*, was from the calx of that metal, which remained after I had dissolved a quantity of it in oil of vitriol in order to procure vitriolic acid air, and of which I had a pretty large quantity; having generally preferred this metal for that purpose. This substance, when it was well dried, was white; but on putting spirit of nitre upon it, and drying it again, it became green. From this substance, thus prepared, making use of a gun barrel, I got about twenty times its bulk of air, of which about one half was fixed air, being readily absorbed by lime water, and making it turbid; and the residuum of the first portion was nitrous air, but that of the last dephlogisticated.

In this manner I procured dephlogisticated air from a calx of copper *previously formed*, pouring spirit of nitre upon it, as I had done before on red lead, and the flowers of zinc; but I afterwards effected

fected the same thing in an easier manner, viz. by distilling to dryness the very solution of copper from which I had just before produced nitrous air; which I at first did with a view to ascertain whether there was any *fixed air* in that solution. It is well known that the solution of copper in spirit of nitre yields only nitrous air; but some considerations had led me to suspect that there might be fixed air in that solution, discoverable by a boiling heat, in the manner in which I had applied it, in order to expel fixed air from water and various other fluids.

With this view I filled a large phial with this solution, having a considerable quantity by me, remaining from most of my experiments on nitrous air; having made it a rule, as much as possible, to throw nothing away, if I could make room for it. No fixed air, however, was procured from this solution in the manner that I had expected; but continuing to make it boil, and supplying it with fresh liquor, as the preceding evaporated, I at length got into the phial all the solid materials of a very large quantity of that solution; and when it approached to dryness, air came from it in very great abundance. I might have caught, I believe several quarts. A considerable quantity I did collect, and found about half of it to be fixed air; and the residuum dephlogisticated. The air was exceedingly
cloudy,

cloudy, and mixed with much red vapour. After the process there remained a black powder.

To this *black powder* I applied more spirit of nitre, when it presently became very hot, and fumed; and being dried again assumed a green colour, attracting much moisture from the air. Being again made thoroughly dry, I put it again into a gun barrel, and with a strong heat, got from it about two pints of air, which I received in three portions, and observed that one half of each was fixed air, the residuum of the first and last nitrous, and that of the middle dephlogisticated. That the last residuum was nitrous, I attribute to the gun barrel, especially as the air came very slowly. After this process very little of the black powder remained.

Having got a quantity of nitrous air from iron, in the usual manner, that is, in a phial with a ground stopper and tube, where the common air had no access to it, I immediately took the residuum, without giving it an opportunity of getting any thing by communication with the external air; and, in a red sand heat got from it about the same quantity of air that it had before yielded of nitrous air. One half of this was fixed air, precipitating lime in lime water, and being readily absorbed by water, and the remainder was altogether nitrous.

It is evident from this experiment that iron contains more phlogiston than copper. The cork, indeed,

deed, which I made use of in extracting this air, was evidently corroded in the experiment, and might contribute a little, both to the fixed and the nitrous air; but the same cork had been used before in the experiment with the copper, which, notwithstanding this circumstance, had yielded the purest air.

Being determined, if possible, to *exhaust* the phlogiston in the calx of iron, by the addition of more spirit of nitre, I repeated the process; and using now a glass phial with a ground stopper, in a sand-heat, as before, I got three ounce measures of air, which was highly dephlogisticated. A small part of this air, however, was evidently fixed air, making lime water turbid.

The bulk of the residuum was still of the same dark brown colour with the common rust of iron; but towards the neck of the phial a part had been sublimed of a light red colour, and above that again was a powder of an exceedingly beautiful yellow, to appearance exactly like sulphur.

I have not made trial of all the *semi-metals*; having hitherto confined my experiments to *zinc*, *bismuth* and *antimony*, all of which yield a considerable quantity of pure air by a direct process, bismuth being not much inferior to zinc in this respect; but I have not made any accurate comparison of them.

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Putting the salt formed by the spirit of nitre and bismuth into a glass vessel, and distilling to dryness, with a sand-heat, I procured from it about twenty times its bulk of air, in the first part of which there was some fixed air; but all the residuum was pure dephlogisticated air, especially at the last.

Having poured a quantity of strong spirit of nitre upon some powder, and small fragments of *antimony*, the whole was reduced, after some days, to a whitish substance, of a pretty thick consistence, without yielding any air. A quantity of this solution I put into a glass vessel, and with a very strong sand-heat I got from it about ten times its bulk of air, about one third of which was fixed air, being readily absorbed by water, and making it turbid, and the remainder was dephlogisticated. Towards the end of the process the proportion of the fixed air was, as is usual, very small, and the dephlogisticated air was of the purest kind,

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SECTION IV.

Of the Production of dephlogisticated Air from the vitriolic Acid and Metals.

AS I have been, perhaps, more than any other person, indebted to what are commonly called accidents (I mean with respect to us; for, in the general plan of nature, and with respect to that great Being who conducts and appoints every thing, there cannot be any such thing as accident) so have I been very often prevented by other accidents from making valuable discoveries, to which I had made near approaches. This was remarkably the case with respect to the production of dephlogisticated air from substances containing the vitriolic acid. For had I, in what I improperly supposed to be an *experimentum crucis*, made use of the calx of perhaps any other metal besides lead, on which the vitriolic acid has no proper action, I could not have failed to hit upon what the better genius of Mr. Landriani brought him acquainted with. Having, as I observed before, got a quantity of red lead which was in a state to give little or no air of itself, I got pure
air

air from it, in great abundance, by means of the nitrous acid, but none at all by means of the vitriolic or marine acid. I therefore concluded, that the nitrous acid, and not either of the other mineral acids, enters into the composition of dephlogisticated, or atmospherical air.

It was not till after I had made the experiments before recited on manganese, and other mineral substances, that I thought of subjecting *green vitriol*, and other saline matters, to the same trial. It is true I had tried them before; but the method was not adequate to the purpose. And though I had even got a small quantity of air considerably better than common air from *Roman vitriol*, I had concluded that "there must certainly have been some nitrous acid in that Roman vitriol." In this case, therefore, as in the experiments with *nitre* and *alum*, I had made a discovery without being sensible of the value of it, or indeed understanding it. Nor, when I resumed my experiments on vitriol, had I any expectation of getting from it any thing besides fixed air and water. However, having every thing at hand, a very slight motive was sufficient to induce me to include this among other articles destined for the same process.

In this manner, therefore, without expecting the actual result, on the 24th of November 1777, I put an ounce of green vitriol into a glass vessel, and
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with a sand heat got from it, at first, after the common air was expelled, and the vapour of the water combined with it was come over, a little fixed air; then, after some interval, a large quantity of vitriolic acid air; the residuum of which was at first hardly perceivable, but was afterwards considerable, and chiefly fixed air. When the residuum was still more considerable, I found that it was diminished by nitrous air. At length it had no mixture of vitriolic acid air, but was very turbid, and appeared to be pure dephlogisticated air, except that, at the last, it was not quite so pure as before. Of this dephlogisticated air, I collected ten ounce measures. What remained in the glass vessel was six pennyweights, of a purplish coloured ochre.

Notwithstanding this evident production of a considerable quantity of dephlogisticated air from green vitriol, which is a combination of iron and the vitriolic acid, I still suspected, as in the case of the Roman vitriol mentioned before, that, by exposure to the common atmosphere, or in some other unknown manner, this vitriol, which had been bought at a common shop, might have got some mixture of spirit of nitre. I therefore made a quantity of vitriol myself, by dissolving iron filings in oil of vitriol, diluted with water. This vitriol, treated as the former had been, yielded air of all the same kinds, and in the same proportions, as in the preceding

ceding experiment; the dephlogisticated air, as then, being very turbid, and exceedingly pure. The first air that came over was the common air in the vessel, a little phlogisticated. A very small quantity of fixed air was still observed in the residuum of the vitriolic acid air, but none after the dephlogisticated air was procured.

In making the vitriol for all the above mentioned experiments, I had taken care that the crystals should be formed at the bottom of a deep glass vessel, so as to have no visible communication with the external air; and I had also covered the vessel as carefully as I could during the process, and had spent as little time as possible in conveying the vitriol from the vessel in which it was formed into that in which it was to be distilled. I determined, however, to avoid the small objection to which this trifling exposure to the air was liable, and therefore next made the distillation in the same retort in which the solution had been made, and in continuation of the same process, so that all communication with the external air was most effectually precluded.

For this purpose I dissolved six pennyweights four grains of iron in diluted Newman's oil of vitriol, and distilling to dryness in a retort with a long neck, I got from it, after the common air was expelled, a small quantity of fixed air; a prodigious quantity of vitriolic acid air, and likewise about
twenty

twenty two ounce measures of the purest dephlogisticated air.

Being now sufficiently satisfied that pure oil of vitriol would always yield dephlogisticated air with iron, it only remained to try whether the ochre remaining from the former experiment, from which air had been procured, would yield more air with more oil of vitriol, which is the case with red lead and spirit of nitre.

Accordingly, I put more oil of vitriol to this residuum (observing that it became very hot by this mixture, as red lead does with spirit of nitre) and then, with a red heat, in a glass retort, it yielded a quantity of vitriolic acid air, no fixed air, but twenty four ounce measures of dephlogisticated air; when, the retort being melted, a good deal of the air was necessarily lost; for the produce of air had not begun to slacken when this accident happened; and removing the retort from the fire, I found only about half of the matter turned red, while the remainder was white. From this circumstance I concluded, that before I had not got more than half the air that it would have yielded. Resuming the process in a gun barrel, I actually got about as much air as I had done before.

I had not now the least doubt remaining but that the acid of vitriol, at least with iron, is capable of properly *generating* dephlogisticated air, as well as

the acid of nitre with lead, or any other substance whatever.

To complete my experiments on the vitriolic acid and iron, I took half an ounce of the common rust of iron, such as is used by apothecaries; and pouring upon it a quantity of that acid, observed that it imbibed it very eagerly, and became of a dark and almost a black colour. Then using a gun barrel, I got from it two or three pints of air; all of which was fixed air, but with a large residuum, about a third of the whole, phlogisticated air.

As the common rust of iron contains a good deal of phlogiston, I did not expect any better result from this experiment. But having, in some measure, purified it by this process, I put more oil of vitriol to what remained of the rust of iron, and then I got from it only a little fixed air, and sixteen ounce measures of dephlogisticated air.

Having got an indisputable production of pure air from *iron*, by means of the vitriolic acid, it was natural for me to proceed to similar experiments on *other metals*, with the same acid. And, in the first place, I made the proper trials with the two remaining kinds of vitriol, the *blue*, into which copper enters, and the *white*, which is composed of zinc; and having now no doubt remaining with respect to the purity of the vitriolic acid which enters into the composition of these kinds of vitriol, I contented myself

myself with specimens bought at the shops, and did not think it necessary to take the trouble to compose them myself.

From about half an ounce of blue vitriol, in a glass vessel, I got a little fixed air, and one ounce measure of dephlogisticated air. The vessel breaking, I put the materials into a gun barrel, and then got from them about twenty five ounce measures of dephlogisticated air, with hardly any more fixed air. The greatest part of this air was very turbid.

To finish my experiments on the three vitriols, I took an ounce of calcined *white vitriol*, and, with a gun barrel, I got from it a great quantity of vitriolic acid air, some fixed air, with five ounce measures of dephlogisticated air. At another time, from one ounce of this kind of vitriol, but uncalcined, I got only about two ounce measures of air, part of which was fixed air, and part dephlogisticated air; not reckoning a great quantity of vitriolic acid air, which came, as usual, before the dephlogisticated air.

I did not think it of much consequence to my purpose to go through all the metals with this process, and therefore only made a trial of such as I happened to have at hand.

I dissolved an ounce of quicksilver, purified by agitation in water, in pure vitriolic acid, in a green glass retort. During the distillation to dryness the retort broke; but collecting the materials as well

as I could (in which perhaps one tenth of the whole might be lost) I put them into a fresh retort, and, exposing them to a red heat, got from them a great quantity of vitriolic acid air, a good deal of fixed air, and about fifty ounce measures of dephlogisticated air.

During the process the solution boiled violently in the form of a red liquor, while the upper part of the retort was coated with a whitish sort of matter. As the heat reached this coating, it also became red; and during the whole process that which evaporated was collected on the sides of the retort, and then descended to the bottom, like drops of blood, or red ink, so as to make a very pleasing appearance. After the process, a very little reddish matter remained at the bottom and on the sides of the retort, which, as well as that which was collected at the neck of the retort, became white when it was cold. Very little of the quicksilver was revived.

SECTION V.

*Dephlogisticated Air procured from other Substances,
containing vitriolic Acid.*

FROM an ounce of calcined *alum*, prepared some months before, I got about six ounce measures of air, all quite as good as, or better than, common air, and without any fixed air in it. The process was in a gun barrel, and the residuum of the *alum* was very hard. This I moistened with oil of vitriol, still keeping it hard and dry; and, in a gun barrel, it yielded again two or three ounce measures of air, chiefly fixed air, and at last some that was about as good as common air. After this it was remarkable that this matter absorbed air, perhaps about an ounce measure in all. This I observed twice, and it may be worth while to investigate this circumstance a little farther.

I afterwards made one experiment in order to ascertain the quantity of this pure air, that a given quantity of *alum* could be made to yield. For this purpose I put one ounce and fourteen pennyweights

of calcined alum into an earthen retort, and by means of a reverboratory furnace I extracted from it one hundred ounce measures of air, a small part of which was fixed air, and the rest so pure, that with two equal quantities of nitrous air, the measures of the test were 1.0.

The water in which this air was received was strongly impregnated with vitriolic acid air. This air containing much phlogiston, and in a state in which it can be imparted to air, was, no doubt, the reason why the air in this case was not so pure as that which is obtained from nitre. Otherwise this would be the cheapest and best method of procuring dephlogisticated air.

Collecting what remained of the alum, it weighed fifteen pennyweights, and still had the taste of alum, though not very strongly. More heat would probably have expelled all the acid, and consequently would have procured more dephlogisticated air. Also had the air been quite pure, it would, no doubt, have been much more in quantity. If the weight of the alum, and of the residuum after this process be compared, it will be found that this one hundred ounce measures of air occasioned the loss of only nineteen pennyweights, that is, not quite an ounce of the alum, and since the one hundred ounce measures of dephlogisticated air would weigh about sixty
six

six grains, the weight of the vitriolic acid air, with which the water was impregnated, may be estimated at sixteen pennyweights six grains.

Another time I got sixty ounce measures of air from an ounce of alum, which is in about the same proportion as in the former experiment. But it had been so well calcined previous to this process, that some of the air had probably been expelled in that operation; and still what remained tasted very sensibly of alum. This air being examined, the measures of the test, with two equal quantities of nitrous air, were 1.4. There was hardly the least sensible quantity of fixed air produced.

It is easy to conceive, that till any substance be completely dephlogisticated, it cannot yield dephlogisticated air; and it is something remarkable, that a *red colour* should be the criterion of dephlogistication, both in the calx of iron, and of mercury. Accordingly, when mercury is dissolved in spirit of nitre, the produce is pure nitrous air, not only during the solution itself, but also during the application of heat to the yellow concrete mass, that is formed by the evaporation of the solution; and no dephlogisticated air is produced till the red precipitate is completely formed. But the action of heat upon this red substance is always followed by the production of pure air, as much as from the precipitate per se.

It is also evident from this experiment, that the air produced in it does not come from the atmosphere, which has been conjectured with respect to some of the processes for procuring dephlogisticated air; but must have been contained in the ingredients, viz. spirit of nitre.

To half an ounce of *quick lime*, I put oil of vitriol till it weighed one ounce four pennyweights, when it made a hard mass. This I pounded, and putting it into a gun barrel, I got from it, in all, about ten ounce measures of air, the greatest part of which was fixed air; but towards the last, when the heat was as great as I could make it, in a common fire, urged with a pair of bellows, the residuum was as good as common air, or rather better. This air came over very turbid.

I concluded from many experiments on substances containing marine acid, that it differs essentially from both the vitriolic and nitrous in this, that it cannot, by any combination whatever, be made to yield dephlogisticated air, at least with the degree of heat that I was able to apply. But other persons have procured it by means of this acid, as Mr. Landriani, who first succeeded in this process, and then Mr. Bertholet. I have now procured this air by means of the *acetous acid*, and though the fact is but of little consequence, and the
expe-

experiment was made with another view, it may not be amiss to recite the particulars of it.

Having mixed half an ounce of *radical vinegar* with two ounces of calcined whiting, I got from it, by heat, in an earthen retort, 350 ounce measures of air, of which about one third was fixed air, but more than this in the first portions, and less in the last. The standard of the residuum in the first portions was 1.66, in the second 1.42, and in the third 1.38; which was very near the purity of common air. The whiting then weighed 760 grains. I then put a quarter of an ounce more radical vinegar to it, and without taking any account of the quality of the air, only observed that I got 120 ounce measures from it, and that the residuum weighed 730 grains. After this I put to it a quarter of an ounce more of the same acid, and got from it 140 ounce measures of air, of which the last portion had in it no fixed air, and was considerably better than common air; but of the exact standard of it I find no note. The whiting then weighed 489 grains. The air had always been very turbid, which accounts for the continual loss of weight in these processes.

SECTION VI.

Of the Extraction of dephlogisticated Air from several Mineral Substances.

WITH no other view than to try what kind, and proportion of air, different substances would give in a red heat, I entered upon the examination of *manganese*. Of this I procured a quantity finely pounded; and from an ounce of it I got, in a red sand heat, forty ounce measures of air, part of which, in every portion, was fixed air, and at first almost wholly so; but four fifths of the last was the purest dephlogisticated air. Even the first that came over, which was the common air in the vessel, was not in the least phlogisticated. The manganese had lost one pennyweight and a half of its weight, and was not to be distinguished in colour (which was black) from what it had been before. A considerable quantity of water came over during this process.

That manganese should give *fixed air* did not at all surprize me; since there are few earthy substances that do not contain more or less of it; but I did not at all expect the dephlogisticated air; as,
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before I had imagined that the nitrous acid was necessary to the production of it, or at least the influence of the atmosphere, which I supposed might deposit the acid that entered into its composition, and which I concluded to be the nitrous. On the contrary here was pure air from a substance which, for any thing that appeared, had always been in the bowels of the earth, and never had had any communication with the external air; and yet it exactly resembled *red lead*, both in yielding fixed air, and dephlogisticated air; and it is known that red lead, like the *precipitate per se* cannot be made but in contact with the open air.

I next made trial of some *lapis calaminaris*, first pounding it very finely, then putting an ounce of it into one of the small long necked retorts already mentioned, and with a red hot sand heat I got from it 306 ounce measures of air; and making allowance for the loss of air in changing the vessels in which I received it, &c. I believe I may state the whole produce at 316 ounce measures, the whole of which was fixed air, except four ounce measures; and, what I did not at all expect, the residuum, after the fixed air had been extracted from it by water, appeared to be nearly as good as common air. For one measure of it and one of nitrous air occupied the space of one measure
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and a quarter. Had the proper residuum of fixed air been well extracted, the remainder would probably have been dephlogisticated air. What remained of the lapis calaminaris weighed thirteen pennyweights six grains, and had a lighter colour than before. This produce of air I took at different times, but the residuum of the last portion was but little better than that of the first.

Spirit of nitre made no effervescence, and produced no heat, in mixing with the lapis calaminaris. It also made no change of colour in it, and a very little of the acid was sufficient to make it sensibly moist; in all which respects it differs very remarkably both from red lead, and from manganese.

I next proceeded to the examination of the mineral substance called *wolfram*, from the mines of Cornwall. This I pounded, especially the black part of it, and treating it, in all respects, as I had done the lapis calaminaris, I procured from an ounce of it, not more than about an ounce measure of air, a little of which was fixed air, but the remainder was about the standard of common air. It required a great and long continued heat to extract this air, and I had nearly desisted from the process before any of it came. After the process the wolfram was, to all appearance, the same as before. Perhaps a greater degree
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of heat, in vessels proper for sustaining it, would have produced a greater quantity of air.

These experiments suggested to me that, possibly, the expulsion of dephlogisticated air from these, and other mineral substances, might assist in sustaining subterraneous fires. For phlogiston set loose in the dissolution of all bodies by ignition must be received by some other substance, as it is not a thing that, as far as we know, can exist, except in combination with other substances; and we do not know of any thing that can combine with it so readily as *air*; and therefore we find that nothing can burn but in contact with air, and with change of air.

When, indeed, phlogiston is set loose in the putrefactive process, air is not absolutely necessary. For, in that case, it may be communicated to water, and probably to other substances fluid or solid. It does not, therefore, certainly follow, that there can be no combustion without air, though it be probable; because phlogiston may be able to escape without the help of air in one way, though not in another. The solution of the phenomena of subterraneous fires would certainly, however, be much easier on the supposition of their supplying their own *pabulum*, by means of dephlogisticated air, contained in substances exposed to their heat.

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I therefore desired Mr. Landriani, who, being in Italy, had a good opportunity of making inquiries on the subject, to inform me whether any of those substances, and particularly *manganese*, be found in their volcanos; and his answer makes it rather probable that those fires are, in part, sustained by this means. The extract of his letter, translated from the Italian, is as follows.

“ With respect to what you desire to be informed of, concerning the *volcanic productions*, there is found in the *zolfatara* of Pozzuolo a great quantity of *martial vitriol*; but I do not know that there is any *manganese*, or *lapis calaminaris*, found there. The Abbé Fortis, who has lately examined the extinguished volcano of Verona, assures me that, besides *martial vitriol*, he has found a quantity of *manganese* there. Sig. Volta, having repeated the experiments that I communicated to him, has lately informed me, that he has found dephlogisticated air in calcined *roche alum*, a substance which is found in great quantities in all volcanos; so that it is out of doubt, that subterraneous fires are continually fed with dephlogisticated air, dislodged from substances proper for supplying it.”

It is very probable, that other mineral substances may contain dephlogisticated air as well as these;
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and it is certainly very well worth while to add this process to the chemical analyses of them. Whether the substance be converted into air, or whether it contain the air, in a condensed or combined state, like fixed air in chalk, it is still of importance to know what kind of air they may be made to yield by heat; and in time we may be able to ascertain the true origin of such air.

PART

P A R T II.

OF THE PROPERTIES AND USES OF DEPHLOGIS-
TICATED AIR.

SECTION I.

Of Combustion and Respiration in dephlogisticated Air.

I Have not made many experiments on the mixture of dephlogisticated air with the other kinds of air, because the analogy which it bears to common air is so great, that I think any person may know before-hand, what the result of such experiments would be. It is pleasing, however, to observe how readily and perfectly dephlogisticated air mixes with phlogisticated air, or air injured by respiration, putrefaction, &c. each tempering the other; so that the purity of the mixture may be accurately known from the quantity and quality of the

the two kinds of air before mixture. Thus, if one measure of perfectly noxious air be put to one measure of air that is exactly twice as good as common air, the mixture will be about the standard of common air.

I observed also, in making this experiment, that after mixing one measure of each of these kinds of air, they made exactly two measures; so that there was neither any increase nor diminution of quantity in consequence of the mixture, as is the effect of mixing nitrous air with either common or dephlogisticated air.

It may hence be inferred, that a quantity of very pure air would agreeably qualify the noxious air of a room in which much company should be confined, and which should be so situated, that it could not be conveniently ventilated; so that from being offensive and unwholesome, it would almost instantly become sweet and wholesome. This air might be brought into the room in casks; or a laboratory might be constructed for generating the air, and throwing it into the room as fast as it should be produced. This pure air would be sufficiently cheap for the purpose of many assemblies, and a very little ingenuity would be sufficient to reduce the scheme into practice.

My reader will not wonder, that, after having ascertained the superior goodness of dephlogisticated

air by mice living in it, and the other tests above-mentioned, I should have the curiosity to taste it myself. I have gratified that curiosity, by breathing it, drawing it through a glass syphon, and, by this means, I reduced a large jar full of it to the standard of common air. The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it.

Some of my friends have expressed a little doubt about the certainty of the test of nitrous air, as a measure of the wholesomeness of respirable air in general, and of dephlogisticated air in particular. To this I can only say, that every thing I have yet observed leads me to depend upon the accuracy of this test, with respect to dephlogisticated as well as common air; and, according to what I should think to be the fairest method of computation, dephlogisticated air serves even longer for respiration than, from this method of examining it, I should have conjectured *a priori*.

The most natural method, as I should think, for estimating the purity of air, and thereby judging of the *time* that any given quantity of it would suffice

suffice for the purpose of respiration, would be to find the quantity of phlogiston that is required to saturate it, or which comes to the same thing, the quantity of nitrous air that is required to bring it to the state of perfectly phlogisticated air. But a mouse will live much longer in a given quantity of dephlogisticated air, than in this proportion, with respect to common air; owing, I suppose, to the animal not throwing out equal quantities of phlogiston in equal times, but much less at the last, when the vital powers are languid, than at the first.

I have a glass vessel which I have made use of in all my experiments with mice, from the beginning of my researches into this subject, a considerable time before I had discovered nitrous air. In this vessel, which was a tall beer glass, and which holds about two ounce measures of air when a mouse of a middle size is confined in it, I never knew it to live longer than half an hour, and in general they have not survived twenty minutes. Supposing, however, the full time for a mouse's breathing the common air contained in this vessel to be half an hour, I should not have expected that a species of air which required only *four times* as much nitrous air to saturate it, would suffice for the respiration of the same animal more than four times as long; and therefore that in this

vessel of dephlogisticated air, which at a medium requires about that proportion of nitrous air to saturate it, a mouse might live about two hours. But I believe that mice in general will live considerably longer in that quantity of dephlogisticated air.

I lately put a young mouse into that very vessel, filled with dephlogisticated air, such as when mixed with two equal quantities of nitrous air, the measures of the test were 0.55. It continued there near three full hours; and being taken out alive, the air was found to be so far from being phlogisticated, that it was still considerably better than common air; for, with an equal quantity of nitrous air, the measures of the test were 1.05. Perhaps this mouse being languid, in consequence of its confinement, did not phlogisticate the air so fast as it would have done had it been more vigorous. But then this may in general be expected to be the case with all mice, in the same situation.

I once observed, that I could never make mice live in dephlogisticated air till they had completely phlogisticated it, and I could not, at the time of writing that article, assign any sufficient reason for the fact. Being unwilling to leave this difficulty unsolved, I repeated the experiment; and putting a vigorous mouse into about ten ounce measures of this air, it continued some hours seemingly at its ease,

ease, but died while the air was so pure, that, with two equal quantities of nitrous air, the measures of the test were considerably less than 1.0.

I then put another young mouse into the remainder of the air, and it also continued at its ease two or three hours; but then seemed to be expiring, its respiration being very languid, and so slow, that I several times concluded it was absolutely dead. I did not at first suspect that it could be affected by *cold*, when other mice lived very well in a wire cage in the same room; for it had soon become quite dry after passing through the water, and had never shewn any sign of uneasiness. But bringing it near the fire, when the heat was about eighty or ninety degrees (though I removed it occasionally when it seemed to be uneasy on that account) it lived several hours longer, and when it died the air was as completely phlogisticated as common air is generally found to be when mice have died in it.

This experiment fully satisfied me, that it was nothing in the dephlogisticated air itself that was the reason that mice could not live in it. I observed before, that a mouse is a tender animal, and after passing through water, requires a considerable degree of warmth; but I did not suspect that it required so much, and such a continuance of it, as I found in this particular case.

I easily conjectured that inflammable air would explode with more violence, and a louder report, by the help of dephlogisticated than of common air; but the effect far exceeded my expectations, and it has never failed to surprize every person before whom I have made the experiment.

Inflammable air requires about two thirds of common air to make it explode to the greatest advantage; and if a phial, containing about an ounce measure and a half, be used for the experiment, the explosion with common air will be so small, as not to be heard farther than, perhaps, fifty or sixty yards; but with little more than one third of highly dephlogisticated air, and the rest inflammable air, in the same phial, the report will be almost as loud as that of a small pistol; being, to judge by the ear, not less than forty or fifty times as loud as with common air.

The orifice of the phial in which this experiment is made, should not much exceed a quarter of an inch, and the phial should be a very strong one; otherwise it will certainly burst with the explosion. The repercussion is very considerable; and the heat produced by the explosion very sensible to the hand that holds it. I have sometimes amused myself with carrying in my pocket, phials thus charged with a mixture of dephlogisticated and inflammable air, confined either with common corks or
ground

ground stoppers, and I have perceived no difference in the explosion, after keeping them a long time, and carrying them to any distance.

The dipping of a lighted candle into a jar filled with dephlogisticated air is alone a very beautiful experiment. The strength and vivacity of the flame is striking, and the heat produced by the flame in these circumstances is also remarkably great. But this experiment is more pleasing, when the air is only little more than twice as good as common air; for when it is highly dephlogisticated, the candle burns with a crackling noise, as if it was full of some combustible matter.

It may be inferred, from the very great explosions made in dephlogisticated air, that, were it possible to fire gun-powder in it, less than a tenth part of the charge, in all cases, would suffice; the force of an explosion in this kind of air, far exceeding what might have been expected from the purity of it, as shewn in other kinds of trial. But I do not see how it is possible to make this application of it. I should not, however, think it difficult to confine gun-powder in bladders, with the interstices of the grains filled with this, instead of common air; and such bladders of gun-powder might, perhaps, be used in mines, or for blowing up rocks, in digging for metals, &c.

Nothing, however, would be easier than to augment the force of fire to a prodigious degree, by blowing it with dephlogisticated air instead of common air. This I have tried, in the presence of my friend Mr. Magellan, by filling a bladder with it, and puffing it, through a small glass tube, upon a piece of lighted wood; but it would be very easy to supply a pair of bellows with it from a large reservoir.

Possibly much greater things might be effected by chemists, in a variety of respects, with the prodigious heat which this air may be the means of affording them. I had no sooner mentioned the discovery of this kind of air to my friend Mr. Michell, than this use of it occurred to him. He observed that possibly *platina* might be melted by means of it. These conjectures have been abundantly verified by the experiments of Mr. Lavoisier and others.

From the greater strength and vivacity of the flame of a candle, in this pure air, it may be conjectured, that it might be peculiarly salutary to the lungs in certain morbid cases, when the common air would not be sufficient to carry off the phlogistic putrid effluvium fast enough. But, perhaps, we may also infer from these experiments, that though pure dephlogisticated air might be very useful as a *medicine*, it might not be so proper for us in the usual

usual healthy state of the body; for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, *live out too fast*, and the animal powers be too soon exhausted in this pure kind of air. A moralist, at least, may say, that the air which nature has provided for us is as good as we deserve.

Whether the air of the atmosphere was, in remote times, or will be in future time, better or worse than it is at present, is a curious speculation; but I have no theory to enable me to throw any light upon it. Philosophers, in future time, may easily determine, by comparing their observations with mine, whether the air in general preserves the very same degree of purity, or whether it becomes more or less fit for respiration in a course of time; and also, whether the changes to which it may be subject are *equable*, or otherwise; and by this means may acquire *data*, by which to judge both of the past and future state of the atmosphere. But no observations of this kind having been made in former times, all that any person could now advance on this subject would be little more than random conjecture. If we might be allowed to form any judgment from the length of human life in different ages, which seems to be the only *datum* that is left us for this purpose, we may conclude
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that, in general, the air of the atmosphere has, for many ages, preserved the same degree of purity. This *datum*, however, is by no means sufficient for an accurate solution of the problem.

SECTION II.

Of the very great Purity of some dephlogisticated Air.

IT appears from my first observations on the properties of dephlogisticated air, that, in general, that when two equal measures of nitrous air are mixed with one measure of it, the whole is reduced to half a measure, and sometimes, when I thought it peculiarly pure, to one sixth of a measure. I have since, on one particular occasion, produced this kind of air in a state of so much greater purity as appeared very extraordinary to myself, and I doubt not will be thought so by others.

Having, for a purpose that has been mentioned in the account of my observations on fixed air, kept a solution of mercury in spirit of nitre for several months,

months, in a phial with a ground stopper, I put it into a retort with a long neck, and, in a sand heat, received in the first place, the nitrous air it yielded, and then without removing the retort from the fire, the dephlogisticated air. Using both the nitrous and dephlogisticated air of the same product, I observed that two measures of the former and one of the latter mixed together, occupied, after the effervescence was over, the space of no more than three hundred parts of a measure.

It was impossible for me to be mistaken with respect to this remarkable fact; for the tube in which I measured the residuum was so long, in proportion to the capacity of the phial which I used as a measure, that a hundredth part of a measure exceeded the eighth of an inch. Repeating the experiment, I found that two measures of nitrous air were rather more than sufficient to saturate one measure of the dephlogisticated air; so that, possibly, had the former experiment been made with more circumspection, the diminution, extraordinary as it was, would have been somewhat greater. Indeed it cannot be supposed, that *exactly* two measures of nitrous air should be the precise quantity that would produce the greatest diminution. It should also be considered, that a small portion of air might be yielded by the water in which the experiment was made.

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21 Upon the whole, therefore, I am inclined to think that, were it possible to make both the nitrous and dephlogisticated air in the greatest purity, and then to mix them in some exact proportion, the aerial form of them both would be entirely destroyed, the whole quantity seeming to disappear, as in the mixture of alkaline and acid air. But whereas a white saline substance is the immediate visible result of this mixture, there is no visible produce from the other, the whole, whatever it be, being dissolved in the water; so that, this would probably be the more striking phenomenon of the two; and the mixture of acid and alkaline air never fails to excite a good deal of astonishment, especially when they are previously made, and contained in separate vessels, and then suddenly mixed together, by transferring them from one vessel to another in a trough of quick-silver.

S E C.

SECTION III.

*Of procuring dephlogisticated Air in large Quantities,
and especially from Nitre.*

IMAGINING that the discovery of dephlogisticated air might prove a very useful one, if a quantity of it could be made very *cheap*, as it would be easy enough to contrive methods, by which it might be breathed, provided it should be thought adviseable, in certain morbid states of the lungs and animal system; I have had various schemes for producing it in large quantities, by cheap processes; but none that I have tried have as yet answered. I do not, however, despair; and some person of a happier invention, giving that attention to the subject which it seems to deserve, may be more fortunate.

The cheapest method of procuring dephlogisticated air in large quantities that we are yet acquainted with, is to extract it from nitre by means of heat; and in general no more has been got from an ounce of nitre than about 100 ounce measures of air. The reason of which has been, that, in order to get it pure, *glass vessels* have been used; and after a part
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of the acid has been expelled, the remaining alkali has dissolved the glass, and the process has of course terminated. But by means of Mr. Wedgwood's excellent earthen retorts, I have been able to get such quantities of this air from nitre, as to myself, and all my friends, appeared very extraordinary.

From two ounces of nitre, in an earthen retort, and with an intense white heat, raised by such a furnace as Dr. Black has constructed, I got at first five hundred ounce measures of air, the whole considerably dephlogisticated, and with very little fixed air. With the first produce the measures of the test were 0.7; but the last part of the produce came slowly, and the measures were 1.3, which shews that the air was considerably injured by something in the retort. For the air procured by this process in glass is more pure, the measures of the test being generally less than 0.5.

What remained in the retort was a dark green or blue substance, extremely acrid to the taste, and deliquescent, weighing, when it was taken out of the retort, about eighteen pennyweights. The air would have weighed about thirteen pennyweights, and the water of crystallization, which is eighteen parts in 110 of the water, six pennyweights and a half, so that three pennyweights remain to be accounted for of the two ounces. This last was, in part, an *acid vapour* diffused through the air, and not

not incorporated with it; for whenever I emptied any of the vessels of this air, I perceived a very pungent smell. Part of the loss also was the *white cloud* with which the air was often filled when it was produced.

From the same quantity of *purified nitre*, in one of Mr. Wedgwood's retorts, I got 787 ounce measures. The air procured in this manner is never of the purest kind; and the standard of this, from the first to the last, with two equal measures of nitrous air, was only 1.25. I conducted this process so that the air was very little turbid, and yet there was something acid discovered by the smell in every portion of it. In another process I got 796 ounce measures of air, and the retort was dissolved before the process was completed. But the largest quantity that I ever got from two ounces of nitre was 812 ounce measures, and this was purer than any I had got before in this way, the standard of the greatest part of it being 0.95, though at the very last 1.5. In all cases I have observed, that the purer the air is, the greater is the quantity. For it can only be rendered impure by some phlogistic matter uniting with the purest part of it, and forming, in many cases, as I am inclined to think, phlogisticated air. In this experiment the loss in the nitre was 535 grains. The air would have weighed something more than 487 grains, so that more than one half

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of

of the weight of the nitre was reduced to the form of air; and there remained thirty four grains for the water of crySTALLIZATION, &c.

In these experiments it was a matter of some consequence to determine whether any *acid* remained in the nitre after all the air was come over; and as the vitriolic acid has a stronger affinity with the base of the nitre, there appeared to be no better method of determining this, than to put oil of vitriol to it, and endeavour to make nitrous acid. Accordingly, in the former processes, I had always been able to get nitrous acid, or nitrous air, from what remained of the nitre, after the dephlogisticated air was expelled from it. Thus from half an ounce of the saline residuum which remained from one of the preceding processes, I got nine ounce measures of nitrous air.

To these circumstances I gave particular attention in my future experiments. To that which remained of the last mentioned experiment, in which I procured 812 measures of air from two ounces of nitre, I put oil of vitriol and water. But though it was exposed to heat till the water was distilled over, I did not get half an ounce measure of air more than the vessel contained; and this was dephlogisticated. Consequently, all the nitrous acid had been expelled from this nitre.

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The colour of the alkaline matter to which the nitre was reduced in these earthen retorts, was a deep green, owing, perhaps, to something of the earth in the retort; for in a porcelain vessel, I, at one time, had it perfectly white.

At Mr. Watt's request, I endeavoured to ascertain the quantity of acid that was expelled from nitre, in procuring the dephlogisticated air from it. To do this, I put two ounces of purified nitre into a glass retort, and receiving the air in 300 ounce measures of water, only filled each recipient half full, and agitated the air very much in the water, in order to make the water imbibe as much as possible of the acid that it contained. However, notwithstanding this agitation, every vessel of the air retained a strong smell of the acid.

I afterwards examined a portion of this water, by expelling air from it, and did not find it to contain more air than water generally does, but what I did get from it was considerably phlogisticated.

SECTION IV.

*Of the White Matter deposited in the Production of
dephlogisticated Air.*

IN the rapid production of all kinds of air from earthy materials, I have frequently observed that there is a quantity of superfluous *white matter* deposited in the cold water in which it is received. This earth seems to have been held in solution in the air while it was hot, because it was then quite transparent, and did not become turbid till it was cool; and this was one reason why I once thought that an earth is the proper basis of all such kinds of air. For if some earth be certainly held in a proper solution, so as to make a constituent part of the air while hot, as its transparency seems to prove, and it be only deposited by *cold*, some of the earth must, I thought, be retained by it in every degree of heat, and therefore in the temperature of the atmosphere. And perhaps no degree of cold can deprive it of all the earth that it contains. If it should, I should have imagined that, as nothing but the *acid principle* would remain, it would then, like any other *acid*
air,

air, become liable to be immediately absorbed by water.

This earthy matter when incorporated with the air, I imagined to be then *the same thing*, from whatever substance the air had been produced, being divested of every thing that was peculiar to the substance from which it had been expelled; just as the *acidifying principle*, in the composition of dephlogisticated air, is probably the same thing, whether the air had been produced from materials containing spirit of nitre, or oil of vitriol. If this reasoning be true, we should, I thought, be in possession of a method of obtaining a truly *primitive earth*, or an *earthy principle* common to all earths, and all metallic calces whatsoever, since dephlogisticated air may, as I had shewn, be produced from them all. The following observations, however, may perhaps lead to a contrary conclusion, or, that earth deposited from dephlogisticated air produced from different materials, has not, in all respects, the same properties.

Having collected some of the white powder diffused through a quantity of dephlogisticated air, procured from minium and spirit of nitre, I observed that when it was dry, it was of a grey colour, and that it was not, at least immediately, affected by spirit of salt. When it was heated in a glass tube, by means of the flame of a candle and a blow-pipe, it fumed copiously, and covered the inside of

the tube with a white substance; that which was not sublimed becoming black. When it was laid upon a red hot iron, it smoked very much, and became of a brown colour. But in none of its forms was it quickly affected by spirit of salt, though after twelve hours this acid did acquire an orange colour, both from the black and the brown matter.

At the same time I had by me a quantity of white matter which, as I believe, had been collected in a similar manner, when I procured dephlogisticated air from *red precipitate*; but having lost the label, I could not be absolutely certain. This matter was perfectly white when dry, and bore a red heat without any sensible change, nor was it affected by spirit of salt.

I once collected a quantity of this substance, and found that it continued in the form of a white powder. It made no effervescence with spirit of nitre, but seemed to be in part, though not wholly, dissolved in it.

PART III.

MISCELLANEOUS OBSERVATIONS RELATING TO
DEPHLOGISTICATED AIR.1. *Of the Detonation of Nitre.*

THE discovery of dephlogisticated air throws great light on many very important facts in chemistry, but upon none more than upon that very difficult and striking one, of the *detonation of nitre*, concerning which the most improbable conjectures had been advanced by the most eminent philosophers and chemists. This detonation is the sudden inflammation produced by the contact of various substances containing phlogiston and nitre, when either of them is red hot. The hypothesis that has been thought the most satisfactory is that of

Mr. Macquer, who supposes that, in these circumstances, an union is formed between the pure nitrous acid and phlogiston, similar to that which is formed between the vitriolic acid and phlogiston in the composition of sulphur. He therefore supposes that, in this case, a *nitrous sulphur* is formed, and that this substance is of so inflammable a nature, that it cannot exist a moment without actual ignition.

But I would observe that, supposing this hypothetical nitrous sulphur to be actually formed, yet if it resemble other combustible substances, the vitriolic sulphur for instance, or any other whatever, in a property that is common to them all without exception, it cannot be inflamed but in contact with air; which, according to conclusions clearly drawn from my experiments, and all other observations, is saturated with phlogiston by the process, and when saturated can take no more, let the substance that is heated in it be ever so combustible; and consequently, in those circumstances, all inflammation must be impossible. Whereas Mr. Macquer acknowledges, that this nitrous sulphur is capable of the most violent inflammation in the closest vessels, where there is no access of air, and it is well known that compositions of gunpowder are made to burn even under water.

Now the doctrine of dephlogisticated air supplies the easiest solution imaginable of this very difficult phenomenon.

phenomenon. For it appears that the nitrous acid cannot be heated to a certain degree, without producing dephlogisticated air; by the help of which all combustible substances burn with the greatest violence, much more than they can be made to burn with in common air. Here then, I suppose that the moment the acid of nitre, contained in the nitre, and the earth of the coal, for example, thrown into it become red hot, in contact with each other, dephlogisticated air is produced; and in this air the remainder of the charcoal, being likewise red hot, burns with the violence that is observable in the experiment; while, at the same time, other portions of the nitrous acid are forming more dephlogisticated air; and thus the detonation continues, till all the charcoal, or all the nitre, is consumed; the acid not being *lost*, as some chemists express it, but entering into the composition either of the dephlogisticated air, or of some other kind of air, that may be generated in the process.

Let any person but attend to the phenomena of the detonation of charcoal in nitre, and that of the dipping a piece of hot charcoal into a jar of dephlogisticated air, and I think it will be impossible for him not to conclude that the appearances are the very same, and must have the same cause.

There is the same intense incandescence, and the same rapid consumption of the charcoal in both cases; and this is evidently owing to the eagerness, as I may say, with which this species of air, the most free from phlogiston itself, seizes upon the phlogiston of other bodies, in a sufficient degree of heat. Such appearances cannot be produced in common air, which, being more than half saturated with phlogiston already, can take but little more; and therefore, to produce an appearance any thing resembling them, we are obliged to supply the fire with a current of fresh air thrown into it by bellows. But supplying a fire in the same manner with a current of dephlogisticated air, which I have sometimes done, has a most astonishing effect of the same kind.

I shall conclude this article concerning detonation, with an account of a very striking experiment that I made with Homberg's pyrophorus. I put a quantity of it into one of the small jars which I use for experiments on air in quicksilver; then, filling up the vessel with quicksilver, I inverted it in a basin of the same, and threw up dephlogisticated air at different times. It always occasioned a sudden and vehement accension, like the flashing of gunpowder, and the air was greatly diminished, as might have been foreseen.

2. *Of*

2. *Of the Formation of Precipitate per se.*

Precipitate per se is much more easily procured in dephlogisticated than in common air, and probably not at all in phlogisticated air; this air not being capable of taking any phlogiston from mercury, without which the calx cannot be formed.

I exposed equal quantities of the same quicksilver, in equal glass tubes, of about two feet and a half long, and an inch and a half in diameter, but narrower towards the top, to a sand heat, for one day, one of the tubes containing phlogisticated, and the other dephlogisticated air, both hermetically sealed. In the result, the mercury in the tube containing the dephlogisticated air was completely covered with a coating of precipitate per se; but the mercury in the other tube was not sensibly altered. When the process had been resumed, and continued four days, I opened the tubes, and found the dephlogisticated air something worse than it had been, but by no means so much so as I had expected; but the phlogisticated air was not at all altered. The quantity of precipitate in the dephlogisticated air was trifling.

I then repeated the same experiments with common air, but in two days no precipitate was formed. With more time there probably would have

been some. But this was sufficient for my purpose, viz. to ascertain the difference that would be produced by different kinds of air in this process, according to the quantity of phlogiston which they contained.

Perhaps the *precipitate per se* may, by this means, be made in much less time, and with much less expence, than it now is.

3. *Of the rusting of Metals in Air.*

It is generally thought, I believe, that metals exposed to the open air are corroded, and contract rust, by means of some *acid vapour* contained in it. I thought it possible, however, that very pure air might have such an affinity with phlogiston, as to deprive some metals of it, without the aid of any acid. To try this, I filled an eight ounce phial with very dry clean nails, and then with quicksilver, which I displaced by very pure and dry dephlogisticated air, and left it inverted in a basin of quicksilver on the 13th of April, 1778. On the 26th of January, 1779, I found that one tenth of the whole quantity of air was gone, the quicksilver having risen so high in the phial. I therefore took it for granted, though I could not perceive any rust on the nails, that my conjecture was well founded; that the air has been diminished by
means

means of phlogiston from the iron, and that in *time*, if the quantity should be sufficient, the rust will be apparent. This air being examined on the 20th of July, 1780, was found not to be diminished any farther. It was also a little worse than it had been when it was put into the phial, and the nails were very clean and without rust.

4. *Of the Formation of Nitre.*

Before the discovery of dephlogisticated air it was generally supposed by chemists, that nitrous acid, by which common nitre is formed, exists in the atmosphere as an *extraneous substance*, like water, and a variety of other substances, which float in it, in the form of effluvia; but since there is no place in which nitre may not be made, it may, with more probability be supposed, that nitre is formed by a real *decomposition of the air itself*, the *bases* that are presented to it having, in such circumstances, a near affinity with pure air, which is the principal ingredient in it.

B O O K V.

OBSERVATIONS ON PHLOGISTICATED AIR*.

P A R T I.PROCESSES BY WHICH GOOD AIR IS NOT INJURED,
AND NOXIOUS AIR NOT RESTORED.

ONE of the first objects of my experiments on *atmosphpherical air*, after some relating to *fixed air*, was the cause of the change made in its properties by a variety of processes, which, because I first concluded that the principal agent was phlo-

* It will be seen, that the reason why I called the species of air, which is the subject of this book, *phlogificated*, was my supposition of its being atmosphpherical air, affected by phlogiston. But whether this be the case, or the air of the atmosphere consist of two distinct parts, and substances containing phlogiston, attract one of these, viz. the *dephlogificated*, and leave the other, this other part may still be called *phlogificated air*, so that no mistake will be occasioned by the use of the term.

giston,

giston, I termed *phlogistic*. But before I relate any of them, it may not be amiss to recite the circumstances in which air is not changed, notwithstanding it either had been apprehended, or might naturally be imagined to be so, in them.

SECTION I.

Of Air not being sensibly injured by Heat, or by offensive putrid Substances.

IT had been advanced by the Count de Saluzes, that air is injured by *heat*, and restored by *cold*, especially by frost. But though I made the experiments with all the attention of which I was capable, I did not find these to be the effect of mere heat or cold.

It has been observed both by myself and others, that air exceedingly offensive to the nostrils, is not always properly *phlogisticated*, so as to be distinguished by the test of nitrous air. For though it may be true that phlogiston is the thing that constitutes
smell,

smell, or at least that it is in some manner essential to it, that phlogiston which sensibly affects the olfactory nerves, may be attached to something that is only *diffused* through the air, and not properly *incorporated* with it. For when this air, so exceedingly offensive to the nostrils, is made to pass through a body of water, this phlogiston is entirely separated from it, and leaves the air through which it was diffused, and which it had seemed to contaminate, quite pure and inoffensive.

In order to make full proof of the truth of this observation, and also with the farther view of trying whether the quantity of phlogiston contained in an animal substance might be so far exhausted by putrefying in quicksilver, as to be unable to phlogistificate common air, I confined a large piece of the tendon of a neck of veal, and likewise a whole mouse, in separate vessels of quicksilver, some time in September, 1779; and when they had yielded all the air that I could perceive they would yield, and of which an account has already been given, I took what remained of them both in the April following, and putting them into a jar of common air, containing about seven ounce measures, I examined this air after two days, and did not find it sensibly injured, though the substances were very offensive to the nostrils. After keeping them, however, in the

the same jar about two months longer, I found the air to be phlogisticated.

Notwithstanding this, I make no doubt but that in *length of time* these substances would have lost all their power of phlogisticating air. But whether this property, or that of yielding an offensive smell, would have gone first, I had no opportunity of observing, in consequence of removing my habitation, by which I was obliged to put an end to the process. It appears, however, sufficiently, that very much of the power of these putrefying substances to phlogisticate air was gone before they ceased to be offensive, though it is probable they were not so highly offensive as they had been before.

SEC-

SECTION II.

Of the Air that has been supposed to come through the Pores of the Skin, and of the Effects of the Perspiration of the Body.

IT cannot be thought extraordinary, that when it has been imagined that air is extracted from the most compact bodies, as gold, by means of the air pump, it should be thought to issue from the human skin. It was also very natural to imagine, that since *respiration* injures and phlogisticates air, the *perspiration* of the body, sensible and insensible, should do the same; and they who suppose that phlogiston converts common air into fixed air, must of course imagine, that the air contiguous to the skin is continually undergoing this change. Dr. Ingenhousz asserts the former, and Mr. Cruikshank, after Sig. Moscati, the latter. On both these subjects I shall make some animadversions, and likewise a few experiments which I think will be deemed conclusive, on the subject of perspiration, and sufficient to confirm what I have advanced with respect to it.

Dr. Ingenhousz not only supposes that air is continually issuing from the human skin; but he took pains

pains to collect it, in a considerable variety of circumstances, of which he has given a particular account. This I took the liberty to tell him I had no doubt was a deception; the air that he found not having come from the *skin*, but from the water in which it was plunged; and both the quality of the air that he found, and the circumstances in which he procured it, left me no doubt upon the subject. It was just that mixture of fixed air and partially phlogisticated air, that pump water, which he recommends for the purpose, generally abounds with. The bubbles of air rising and swelling at the same part of the skin is by no means any proof that the air came from the skin; for that is always the case with air issuing from water, the air bubbles never rising within the water itself, but always from some other body immersed in it. All the phenomena he has described may be seen with a piece of metal, or glass, plunged in water containing air, in an exhausted receiver; in which case it is easily shown, that the air does not come from the pores of the metal, or of the glass, but from the water itself. For if the water contain no air, and the surfaces of the metal and of the glass be carefully wiped, that appearance cannot be produced.

He says that water exhausted of its air is not proper for this experiment, because it readily absorbs

all the air as fast as it issues from the skin. But if the experiment be made in water at all, this must be the only unexceptionable manner of making it; and water by no means absorbs any kind of air so fast as he describes this to issue from the skin, and especially such a kind of air as he describes, a great proportion of which is air partially phlogisticated. It requires a long time before water, in a quiescent state, will take up any sensible quantity of such air as this. Besides, there is nothing that we know of the human frame, that would lead any person to suspect that air ever issues from the skin. Where are the *air vessels* for that purpose? and what is their origin, or connexion with other parts of the system? The present state of anatomy indicates nothing on this subject.

To satisfy my friend, not myself, I told him I would make an experiment, which I did not doubt would convince him of his mistake in this respect; I did it in the following manner. I boiled a quantity of rain water, in order to expel from it all the air it might contain, and then sat with my naked arm plunged in a vessel filled with it, after carefully wiping off, under water, all the bubbles of air that adhered to it. But though I continued to sit in this manner a full half hour, not a single bubble of air made its appearance afterwards.

After

After this I need not say any thing to my friend's ingenious observations on the air which he took the pains to collect from the skins of old and young persons, and his laudable endeavours to remove a popular prejudice concerning the unwholesomeness of the former, and the wholesomeness of the latter kind of air.

Mr. Cruikshank's experiments, if they could be depended upon, would both prove that fixed air is composed of common air and phlogiston, and that the perspiration of animal bodies, in a healthy state, has the same effect upon air that breathing it has, viz. phlogisticating it, and making it noxious, which is contrary to the experiments of which I shall presently give an account; by which it appears that the air under my arm-pits, and near other parts of my body, was never less pure than the external air. The Abbé Fontana also told me, that he had always found the same result in experiments made upon himself. But Mr. Cruikshank says (in the second edition of his *Letter to Mr. Clare*, printed in Mr. Clare's Treatise on Abscesses) that, after he had confined his leg in a glass vessel, so as to prevent all communication with the external air, lime water poured into it immediately afterwards, came out a little turbid. But this he would probably have found to be the case with a

small quantity of lime water, poured into, and out of, any vessel of the same size, on account of the great surface of the fluid that must, in those circumstances, have been exposed to the common atmosphere; in consequence of which it is always known to attract fixed air.

However, partly to examine this matter more thoroughly, and with a variation that I had thought of, I repeated the experiments on my own perspiration in various ways, and they all confirmed what I advanced before, viz. that the perspirable matter has no such effect upon the air, but leaves it as wholesome, that is, as fit for respiration, as ever, judging by the test of nitrous air, which, however, Mr. Cruikshank does not say that he ever applied in this case.

Pursuing his steps, I fastened a moist ox's bladder, containing about a quart of air, close about my ankle, so that my foot, clean washed and warm, as his was, was exposed to it; and I sat near the fire, so as to keep my foot properly warm a full hour. After this I carefully withdrew my foot from the bladder, without changing the air; and applying the test of nitrous air, the air in the bladder appeared to be of the same degree of purity with the external air; the measures of the test, applied in the same manner to both, being 1.26. I
also

also admitted part of this air to lime water, and observed that it did not make it in the smallest degree turbid.

Willing to give more time to this experiment, that the opportunity of this perspiration phlogisticating the air might be the greater, I once more fastened the bladder about my foot, just before I went to bed, and slept with it all night, keeping myself sufficiently warm, from eleven to half past six in the morning, when the bladder was quite dry. However, carefully moistening it, and especially where it was fastened to my ankle, I withdrew my foot, without changing the air, and immediately examined it. The quantity contained in the bladder was forty ounce measures. It did not affect lime water, and with respect to purity was of the same standard with common air; the measures of the test with the nitrous air I happened to make use of, being in both cases 1.3.

I cannot therefore but see reason to conclude, as I did before, that it is only *respiration*, and not the *perspiration* of the body that injures common air.

I have sat an hour with my arm in a trough of very warm water, and my warm hand in a glass jar placed with its mouth in the water (my hand, of course, perspiring, though insensibly, all that time) but when I examined the air within the

jar immediately afterwards, it appeared not to have been the least injured by the process.

But what I expected to produce a much more sensible change in the air was the perspiration under the arm-pits, after walking, or using much exercise. For this purpose, I have sometimes taken phials of warm water, and poured it out, when I had introduced my hand as carefully as possible into the place; but at other times I have put open phials, with perforations in the bottoms, and also open glass tubes, three or four inches long, the orifices of which were such as that I could easily cover them with my thumb or finger. This appearing to be the fairest method of all, I made the greatest use of it. For the air within the open tube must certainly, in the course of an hour or two, become of the same quality with the air on the outside of it. In these trials also, I have preferred *walking* to any other kind of exercise, though I have tried several methods; because, in walking, little or no motion is given to the air about the arm; and it is very easy to introduce one's hand, and, covering both the ends of the tube at the same time, to be quite sure that the air within the tube is in that state to which the perspiration of the body had reduced it. But still, after walking a long time, and making myself purposely as hot as possible, I have never found the air within the tubes

tubes in the least degree worse than the external air; but, as I have sometimes thought, a little better.

The experiment of this kind that I made with the most care was in pretty hot weather, on the 4th of June, 1778. I put such tubes as I have mentioned above under each of my arms, and after first working with a spade, and then walking about three miles, in which exercises I purposely made myself exceedingly hot, I withdrew the tubes with as much care as a good deal of experience had taught me, and I found that one measure of this air and one of nitrous air occupied the space of 1.267 measures; when the same experiment being made with the best external air on the same day, the measures were 1.28. Every circumstance in the application of the test was, as near as I could make it, the very same in both cases.

SECTION III.

Of Air which has been exposed to Steam, and of that which is found in the hollow Parts of some Plants.

VERY early in the course of my observations concerning air, I found that the agitation of any kind of noxious air in *water* purified it to a certain degree, as also that the agitation of pure air in water depraved it so much, as to bring it to about the same standard, viz. that in which a candle just goes out. It might, therefore, be thought, that *steam*, or the vapour of water, intimately diffused through a quantity of noxious air, would much sooner imbibe the phlogiston with which it was charged; and several persons have even thought that the melioration of air by vegetation may be owing to the exhalation of moisture from plants in a vegetating state. I was very willing to adopt that idea myself, in preference to my own, which was that plants imbibe the phlogiston with which the air is overcharged *into their substance*, and convert it into their proper nourishment. But when I tried the effects of steam on phlogisticated air, with as much attention as I could give

give to the experiments, I never found that it was at all mended by the process.

I first took a quantity of air that had been phlogisticated by a mixture of iron filings and sulphur, and introducing into it the end of a glass tube, communicating with a phial, which I had filled with water, I kept it in a boiling heat, about a quarter of an hour, in which time the steam had effectually pervaded the mass of air, having made the jar in which it was contained thoroughly hot, and having expelled three fourths of it. But what remained of this air was no more diminished by nitrous air than it had been before.

Afterwards I several times filled jars with air phlogisticated with nitrous air, and also by other means; and placing them, inverted, in pans of water, made the water boil a long time, till a great part of the air was expelled by the steam, but I never found the air so exposed to steam to be at all mended by it. Common air was always sensibly injured by this process, as might have been expected from my former experiments.

I am willing to think, however, from the observation of Mr. Arden, an intelligent lecturer in natural philosophy, who first mentioned the observation to me, as his own, that steam, or the vapour of water, may unite with something or other that makes air offensive, and help to sweeten it; or, at least,

least, that throwing a quantity of steam into a room in which the air is offensive, may promote a change in the air, so as to be an easy and valuable remedy in such cases. He mentioned to me several experiments of his own, as well as the observations of other persons, that make it very probable.

Having examined the state of the air confined in the *bladders of fishes*, I had the curiosity to examine that which was confined in certain hollow places in plants, where it has no visible access to the external air. For this purpose I tried the air contained in the pods of the *bladder senna*, and also in the stalks of *onions*, but did not find that in either of these cases, it was at all sensibly different from the common air; being diminished by nitrous air in the same proportion.

P A R T II.

OF PROCESSES BY WHICH AIR IS RENDERED
UNFIT FOR RESPIRATION OR COMBUSTION.

S E C T I O N I.

*Of Air exposed to a Mixture of Sulphur and Filings
of Iron, to Liver of Sulphur and Pyrophorus.*

READING in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which had been exposed, a mixture of powdered sulphur and filings of iron, made into a paste with water, I repeated the experiment, and found the diminution greater than I had expected. This diminution of air is made as effectually, and as expeditiously, in quicksilver as in water ;

water; and it may be measured with the greatest accuracy, because there is neither any previous expansion, or increase, of the quantity of air, and because it is some time before this process begins to have any sensible effect. This diminution of air is various; but I have generally found it to be between one fifth and one fourth of the whole.

Air thus diminished is not heavier, but rather lighter than common air.

I conclude that the diminution of air by this process is of the same kind with the diminution of it in the other cases, because when this mixture is put into air which has been previously diminished, either by the burning of candles, by respiration, or putrefaction, though it never fails to diminish it something more, it is, however, no farther than this process alone would have done it. If a fresh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I once observed, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had stood for several days, it was quite cold and black, as it always becomes in a confined place; but it presently grew very hot, smoked copiously, and smelled very

very offensively; and when it was cold, it was brown, like the rust of iron.

Air diminished by this mixture of iron filings and sulphur, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The smell of it is at first very pungent and offensive, which must be owing to a quantity of vitriolic acid air generated in the process.

The quantity of this mixture which I made use of in the preceding experiments, was from two to four ounce measures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce measures) was as great with the smallest, as with the largest quantity. How small a quantity is necessary to diminish a given quantity of air to a *maximum*, I have made no experiments to ascertain.

As soon as this mixture of iron filings with sulphur and water, begins to ferment, it also turns black, and begins to swell, and it continues to do so, till it occupies twice as much space as it did at first. The force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immersed in water, it generates no air, though it becomes black, and swells.

Common

Common air, I find, is diminished, and rendered noxious, by *liver of sulphur*. The diminution in this case was one fifth of the whole, and afterwards, as in other similar cases, it made no effervescence with nitrous air.

I found also, after Dr. Hales, that air is diminished by *Homburg's pyrophorus*.

SECTION II.

Of Air infected with the Fumes of burning Charcoal.

AIR infected with the fumes of burning charcoal is well known to be noxious; and Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red-hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a *destruction* of common air as Dr. Hales imagined to be the consequence of burning.

ing. Mr. Cavendish also observed, that there had been a generation of fixed air in this process, but that it was absorbed by soap leys. This experiment I also repeated, with a small variation of circumstances, and with nearly the same result.

I also endeavoured to ascertain in what manner air is affected with the fumes of charcoal, by suspending bits of charcoal within glass vessels, filled to a certain height with water, and standing inverted in another vessel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminished a given quantity of air one fifth, which is nearly in the same proportion with other diminutions of air.

If, instead of pure water, I used *lime water* in this process, it never failed to become turbid by the precipitation of the lime, which could only be occasioned by fixed air, either discharged from the charcoal, or formed by something escaping from it, and something in the common air.

There was the same precipitation of lime, in this process, with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very considerable degree of heat, there never failed to be a permanent addition of inflammable air produced; which agrees with what I had observed before, that, in converting
dry

dry wood into charcoal, the greatest part is changed into inflammable air.

To make the preceding experiment with still more accuracy, I repeated it in quicksilver; when I perceived that there was a small increase of the quantity of air, probably from a generation of inflammable air. Thus it stood without any alteration a whole night, and part of the following day; when lime water being admitted to it, it presently became turbid, and, after some time, the whole quantity of air, which was about four ounce measures, was diminished one fifth, as before.

Air thus diminished by the fumes of burning charcoal, not only extinguishes flame, but is in the highest degree noxious to animals; it makes no effervescence with nitrous air, and is incapable of being diminished any farther by the fumes of more charcoal, by a mixture of iron filings and sulphur, or by any other cause of the diminution of air that I am acquainted with.

This observation, which respects all other kinds of diminished air, proves that Dr. Hales was mistaken in his notion of the *absorption* of air in those circumstances in which he observed it. For he supposed that the remainder was, in all cases, of the same nature with that which had been absorbed, and that the operation of the same cause would not have

failed

failed to produce a farther diminution ; whereas all my observations shew that air, which has once been fully diminished by any cause whatever, is not only incapable of any farther diminution, either from the same or from any other cause, but that it has likewise acquired *new properties*, most remarkably different from those which it had before, and that they are, in a great measure, the same in all the cases. These circumstances give reason to suspect, that the cause of diminution is, in reality, the same in all the cases.

SECTION III.

Of the Effect of the Calcination of Metals.

HAVING been led to suspect, from the experiments which I had made with charcoal, that the diminution of air in that case, and perhaps in other cases also, was, in some way or other the consequence of its having more than its usual quantity of phlogiston, it occurred to me, that the cal-

cination of metals, which are generally supposed to consist of nothing but a metallic earth united to phlogiston, would tend to ascertain the fact, and be a kind of *experimentum crucis* in the case.

Accordingly, I suspended pieces of lead and tin in given quantities of air, in the same manner as I had before treated the charcoal; and throwing the focus of a burning mirror or lens upon them, so as to make them burn copiously, I presently perceived a diminution of the air. In the first trial that I made, I reduced four ounce measures of air to three, which is the greatest diminution of common air that I had ever observed before, and which I account for, by supposing that, in other cases, there was not only a cause of diminution, but causes of addition also, either of fixed or inflammable air, or some other permanently elastic matter, but that the effect of the calcination of metals being simply the escape of phlogiston, the cause of diminution was alone and uncontrouled. It appears, however, from the experiments of Mr. Lavoisier, that the dephlogisticated part of common air is imbibed in this process, and from my own later experiments, that the phlogiston of the metals uniting with another part of it makes fixed air.

The air, which I had thus diminished by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it
(or

(or at least the attempt to make a farther calcination) had no farther effect upon it. This air also, like that which had been infected with the fumes of charcoal, was in the highest degree noxious, made no effervescence with nitrous air, and was no farther diminished by the mixture of iron filings and sulphur.

It might be suspected that the noxious quality of air in which *lead* was calcined, might be owing to some fumes peculiar to that metal; but I found no sensible difference between the properties of this air, and that in which *tin* was calcined.

The *water* over which metals are calcined acquires a yellowish tinge, and an exceedingly pungent smell and taste, pretty much (as near as I can recollect, for I did not compare them together) like that over which sulphur has been frequently burned. Also a thin and whitish pellicle covered both the surface of the water, and likewise the sides of the phial in which the calcination was made; insomuch that, without frequently agitating the water, it grew so opaque by this constantly accumulating incrustation, that the sun-beams could not be transmitted through it in a quantity sufficient to produce the calcination.

Lime-water never became turbid by the calcination of metals over it, the calx immediately seizing the fixed air that was formed, in preference to the lime in the water; but the colour, smell, and taste

of the water were always changed, and the surface of it became covered with a yellow pellicle, as before.

When I was making the experiments on the extraction of inflammable air from iron, I found, that if the quantity of air was considerable, the throwing of the focus of the burning lens upon iron-filings inclosed in it, had no other effect than to diminish the air, and make it noxious; which it did to as great a degree as the calcination of lead or tin had done: for after this it made no effervescence, and was no farther diminished, by nitrous air. After this, I make no doubt, but that if the process had been continued a sufficient time, there would have been an increase of the quantity of air, by the production of inflammable air; but the first effect of the discharge of phlogiston from the iron was the phlogisticating and diminishing of the common air.

I even found that air would be injured by having iron confined in it for a considerable time. To try this, I filled a phial, containing common air, full of nails, the 18th of December, 1773, and left it inverted in a vessel of water till the 2d of March, 1775; when I found that it was diminished one-fifth of its bulk, and was not in the least diminished by a mixture of nitrous air; so that it must have been perfectly noxious.

S E C.

SECTION IV.

Of Air in which Candles have burned.

THE diminution of the quantity of air in which a candle, or sulphur, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one sixteenth of the whole; which is one third as much as by animal or vegetable substances putrefying in it, by the calcination of metals, or by any of the other causes of the complete diminution of air, which will be mentioned hereafter.

I first thought, that flame disposed the common air to deposit the fixed air it contains; for if any lime-water be exposed to it, it immediately becomes turbid. This is the case, when wax candles, tallow candles, chips of wood, spirit of wine, ether, and every other substance which I have yet tried, except sulphur, is burned in a close glass vessel, standing in lime-water. This precipitation of fixed air (if this be the case) may be owing to something emitted from the burning bodies, which has a stronger affinity with the other constituent parts of the atmosphere.

If sulphur be burned in the same circumstances, the lime-water continues transparent, but still there may have been the same precipitation, or formation of fixed air; but that, uniting with the lime and the vitriolic acid, it forms a selenetic salt, which is soluble in water. Having evaporated a quantity of water thus impregnated, by burning sulphur a great number of times over it, a whitish powder remained, which had an acid taste; but repeating the experiment with a quicker evaporation, the powder had no acidity, but was very much like chalk. The burning of sulphur but once over a quantity of lime water, will affect it in such a manner, that breathing into it will not make it turbid, which otherwise it always presently does.

Dr. Hales supposed, that by burning sulphur repeatedly in the same quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the case. Indeed, when the ignition has been imperfect in the first instance, a second firing of the same substance will increase the effect of the first, &c. but this progress soon ceases.

In many cases of the diminution of air, the effect is not immediately apparent, even when it stands in water; for sometimes the bulk of air will not be much reduced, till it has passed several times through a quantity of water, which has thereby a
better

better opportunity of absorbing that part of the air, which had not been perfectly detached from the rest. I have sometimes found a very great reduction of a mass of air, in consequence of passing but once through cold water. If the air has stood in quicksilver, the diminution is generally inconsiderable, till it has undergone this operation, there not being any substance exposed to the air that could absorb any part of it.

I could not find any considerable alteration in the specific gravity of the air, in which candles, or sulphur, had burned out. I am satisfied, however, that it is not heavier than common air, which must have been manifest, if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose, I concluded that air, thus diminished in bulk, is rather lighter than common air.

SECTION V.

Of Animal Substances putrefying in Air.

WHEN a mouse putrefies in any given quantity of air, the bulk of it is generally increased for a few days; but in a few days more it begins to shrink up, and in about eight or ten days, if the weather be pretty warm, it will be found to be diminished one sixth, or one fifth of its bulk. If it do not appear to be diminished after this time, it only requires to be passed through water, and the diminution will not fail to be sensible. I have sometimes known almost the whole diminution to take place, upon once or twice passing through the water. The same is the case with air, in which animals have breathed as long as they could. Also, air in which candles have burned out may almost always be farther reduced by this means.

I have put mice into vessels which had their mouths immersed in quicksilver, and observed that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting lime-water to the air, it immediately became

came turbid, and was contracted in its dimensions as usual.

I tried the same thing with air tainted with putrefaction, putting a dead mouse to a quantity of common air, in a vessel which had its mouth immersed in quicksilver, and after a week I took the mouse out, drawing it through the quicksilver, and observed that, for some time, there was an apparent increase of the air perhaps about one twentieth. After this, it stood two days on the quicksilver, without any sensible alteration; and then admitting water to it, it began to be absorbed, and continued so, till the original quantity was diminished about one sixth. If, instead of common water, I had made use of lime-water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime water in a phial be put under a glass vessel standing in water, it will not become turbid, and provided the access of the common air be prevented, it will continue lime water, I do not know how long; but if a mouse be left to putrefy in the vessel, the water will deposit all its lime in a few days.

The air that is discharged from putrefying substances seems, in some cases, to be chiefly fixed air, with the addition of some other effluvium, which has the power of diminishing common air. The
resemblance

resemblance between the true putrid effluvium and fixed air in the following experiment (which was as decisive as I could possibly contrive it) appeared to be very great; indeed, much greater than I had expected. I put a dead mouse into a tall glass vessel, and having filled the remainder with quicksilver, and set it, inverted, in a pot of quicksilver, I let it stand about two months, in which time the putrid effluvium issuing from the mouse had filled the whole vessel, and part of the dissolved blood, which lodged upon the surface of the quicksilver, began to be thrown out. I then filled another glass vessel, of the same size and shape, with as pure fixed air as I could make, and exposed them both, at the same time, to a quantity of lime water. In both cases the water grew turbid alike, it rose equally fast in both the vessels, and likewise equally high; so that about the same quantity remained unabsorbed by the water. One of these kinds of air, however, was exceedingly sweet and pleasant, and the other insufferably offensive. In general, however, I found some portion of the air from putrefying animal substances to be inflammable; and and it was probably the same in this case.

Putrid cabbage, green or boiled, infects the air in the very same manner as putrid animal substances. Air thus tainted is equally contracted in its dimensions,

sions, it equally extinguishes flame, and is equally noxious to animals; but they affect the air very differently, if the heat that is applied to them be considerable.

SECTION VI.

Of the Effect of the Calces of Copper and Iron, and also of Mercury on Air.

SEVERAL properties of *metallic calces* may be discovered by their exposure to the common air. I have made some observations which may be pleasing and satisfactory with respect to those of *copper* and *iron*. They prove that the blue colour acquired by the former, and the red colour acquired by the latter, are owing to the dephlogistication of them. For these colours cannot be assumed by them but in the open air, and the air to which they are exposed is more or less phlogisticated by this means.

I dissolved copper in a solution of sal-ammoniac, and confined the solution in a phial with a ground stopper.

stopper. After a day or two, when the solution was become thoroughly blue, I examined the air within the phial, and found it to be considerably worse than it had been. For one measure of it and one of nitrous air occupied the space of 1.33 measures; when the common air at the same time was diminished by the nitrous air so much, that the same quantities occupied the space of little more than 1.1 measure. At another time I covered a phial containing a quantity of this solution with a small jar standing in a trough of water, and found, after a few days, though not more than half the solution, beginning from the top, had turned blue, that the air to which it had been exposed was almost completely phlogisticated.

Pouring a diluted solution of pearl ashes into a diluted solution of *green vitriol* with a funnel, that the common air within the phial might mix as little as possible with the open air, the precipitate was at first of a light blue; but by exposure to the air it became first of a deep indigo blue, and then a red.

Covering a quantity of this blue precipitate contained in a glass cup, with a glass jar standing in water, I observed that, after two or three days, all the surface of the precipitate, though covered with water, was become red. When I stirred it up, all below the surface was as blue as ever. In this state

state I examined the air, and found it sensibly phlogisticated, though not to a great degree.

Having made another blue precipitate of iron, I poured it into a small retort, and turning it every way, to give all the inside a coating of it, I exposed it to the heat of the fire, till it was become partially red (for I did not perceive it would become wholly so) and, examining the air in the inside, I found that one measure of it and one of nitrous air occupied the space of 1.3 measures; when the same quantities of common air and the same of nitrous air occupied the space of 1.24 measures.

Lastly, to give the calx of iron more time to affect the air, I made the mixture in a phial which I left half full of air; and in a few days the surface of the water was covered with a red pellicle, and some time afterwards the surface also of the precipitate at the bottom of the phial, which had been of a deep blue, was become red. After waiting three weeks, I examined the air, and found it so much phlogisticated, that one measure of it and one of nitrous air occupied the space of 1.55 measures.

Having also coated the inside of a glass tube with the green precipitate, I let it stand near three weeks with its orifice immersed in water, in which time it had become nearly red; and then examining

ing the air, I found no fixed air in it (which might have been suspected to come from the pearl ashes especially; and thus to have injured the air, without any proper phlogistication) and one measure of it and one of nitrous air occupied the space of 1.45 measures. In this experiment, therefore, there was a proper phlogistication of the common air, without its receiving any thing from the alkaline salts.

It is not a little remarkable, that this change of colour will take place though the precipitate be covered with a large body of water. I have found it when it was covered to the depth of eleven inches, which is that of the trough in which I usually make my experiments. It was at first all blue, the next day I found the surface completely red, when the bottom was as deep a blue as ever. This resembles the property of *serum* in my experiments on *blood*. For as that liquor admits phlogiston to pass from the blood to the air, so water permits phlogiston to pass from this precipitated calx to the air, and, as appears from later experiments, permits the calx to become saturated with dephlogisticated air.

The result of these experiments will be different according to the degree of saturation in the solution, and perhaps according to other circumstances.

At the same time that I got the deep blue precipitate, with which I made several of the experiments above-mentioned, I mixed a quantity of the
saturated

saturated solutions, both of the vitriol and of the pearl ashes, in an open jar, and the whole became red at once, without my being able to perceive any previous blue colour at all. Sometimes the precipitate will be white, or grey, especially when the solution of the iron is poured into that of the alkali. In this case the first change is to a very light blue, then to a deeper blue, and lastly to a red.

In the experiments above-mentioned the air became phlogisticated in consequence of the liquor to which it was exposed *acquiring* colour; whereas in the following it was injured at the same time that the liquor *lost* its colour. I took a quantity of spirit of salt made yellow by various impregnations, and then made it colourless by liver of sulphur. After this I inverted the phial with common air in it, and let it stand about a week, observing that in two days it had recovered its original yellow colour; and the air appeared to be so much injured, that one measure of it and one of nitrous air occupied the space of 1.9 measures. The phlogiston that produced this effect came probably from the liver of sulphur.

I accidentally met with a much better method of making the experiment with the calx, by means of lime water, than that mentioned above.

The trough in which I make my experiments on air being at one time very foul, with various
metallic

metallic solutions, especially in consequence of having dissolved iron in spirit of nitre, for the production of nitrous air, and other purposes, and it not having been convenient to change the water, I continued to use it in that state; when casually pouring a little lime water into it, I observed that a precipitate of a very deep blue colour was formed. It was so beautiful, that, having been obliged to leave my experiments, for the sake of a small excursion to Bath, before I saw any farther into it, I remember telling a friend whom I met there, that I thought it possible that I had accidentally discovered a new and cheap method of making Prussian blue. However my dream of a discovery vanished on my return home, when I observed the bottom of my trough covered with a very lively red. But when I turned it up, I found the red was only superficial, and that the precipitate underneath was of as deep a blue as ever.

I then repeated the experiment in small jars, phials, &c. and was much better pleased with the result than when I had made use of a solution of alkali in order to make the precipitate. Here the lime is seized by the acid, as it was before by the alkali; and in both cases the calx of the iron is set at liberty, and deposited in a phlogisticated state. But it readily parts with its phlogiston if pure air be at hand, even though separated from
it

it by a body of water, of, I believe, any depth. These experiments shew that water, though capable of receiving phlogiston, is not capable of retaining it in the presence of air, which appears to have a much stronger affinity with it.

It may not be improper to mention again in this place, in speaking of the effect of metals on air, that iron that has been suffered to rust in nitrous air diminishes common air very fast.

In a subsequent part of this work I shall show that there is no air in quicksilver; as has generally been imagined, and that all the air which is discovered in boiling it in a glass tube, is only that which had been concealed, and compressed, between the quicksilver and the glass. Having for this purpose collected a small quantity of this air, I found it to be common air, being diminished by nitrous air. But the quantity being small, and not having applied a very accurate measure, I afterwards repeated that experiment with more precaution, and find such air to be in some degree phlogisticated.

I first filled a tall thin tube, about an inch in diameter, with quicksilver; and, exposing the upper part of it to a degree of heat that converted it into vapour, in the manner represented Pl. III. fig. 4, and consequently effectually setting at liberty all the air that was confined between the quicksilver

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and

and the glass, I collected and examined that air, and found it not to be diminished by nitrous so much as common air is.

I then repeated the experiment by throwing up a quantity of common air, and exposing it to heat mixed with the vapour of quicksilver, and let it continue in that state four or five hours. After this I perceived that the air was considerably diminished in bulk; and, examining it, I found that one measure of it and one of nitrous air occupied the space of 1.66 measures. The air, therefore, in the former experiment, not having been pure air, is no proof of its having been incorporated with the quicksilver; since common air mixed with it, in the state of vapour, receives phlogiston from it. This proves that mercury, like other metals, imbibes pure air, and by this means would in time become *precipitate per se*.

SEC.

SECTION VII.

Of the Effect of Oils, &c. on Air.

THE preceding experiments on the calcination of metals suggested to me a method of explaining the cause of the mischief which is known to arise from fresh *paint*, made with white lead and oil.

To verify my hypothesis, I first put a small pot full of this kind of paint, and afterwards (which answered much better, by exposing a greater surface of the paint) I daubed several pieces of paper with it, and put them under a receiver, and observed, that in about twenty four hours, the air was diminished between one fifth and one fourth, for I did not measure it very exactly. This air also was, as I expected to find, in the highest degree noxious. It did not effervesce with nitrous air, it was no farther diminished by a mixture of iron filings and sulphur, and was made wholesome by agitation in water deprived of all air.

Air is diminished by a cement made with one half common coarse turpentine, and half bees-wax. This was the result of a very casual observation.

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Having,

Having, in an air-pump of Mr. Smeaton's construction, closed that end of the syphon-gage, which is exposed to the outward air, with this cement (which I knew would make it perfectly air-tight) instead of sealing it hermetically; I observed that, in a course of time, the quicksilver in that leg kept continually rising, so that the measures I marked upon it were of no use to me; and when I opened that end of the tube, and closed it again, the same consequence always took place. At length, suspecting that this effect must have arisen from the bit of cement diminishing the air to which it was exposed, I covered all the inside of a glass tube with it, and one end of it being quite closed with the cement, I set it perpendicular, with its open end immersed in a basin of quicksilver; and was presently satisfied that my conjecture was well founded; for, in a few days, the quicksilver rose so much within the tube, that the air in the inside appeared to be diminished about one sixth.

To change this air I filled the tube with quicksilver, and pouring it out again, I replaced the tube in its former situation; when the air was diminished again, but not so fast as before. The same lining of cement diminished the air a third time. How long it will retain this power I cannot tell. This cement had been made several months before I made

made this experiment with it. I must observe, however, that another quantity of this kind of cement, made with a finer and more liquid turpentine, had not the power of diminishing air, except in a very small proportion. Also the common red cement has this property in the same small degree. Common air, however, which had been confined in a glass vessel lined with this cement about a month, was so far injured that a candle would not burn in it. In a longer time it would, I doubt not, have become thoroughly noxious.

I before observed that air, which had been exposed to the effluvia of the common *red cement*, was injured by it. For this purpose, I had covered all the inside of a phial with it, and placed it, inverted, in a vessel of water. Since that time I have repeated, or rather continued the experiment, letting the same vessel stand about nine months in that situation; when, upon examining it, I found it was diminished one fifth of its bulk, and that it was not at all affected by nitrous air.

Having had so much to do with *red lead*, in the course of my late experiments, I had the curiosity to try what would be the effect of that process by which the *grey calx of lead* is converted into red lead. The simple calcination of lead, I knew to be the discharge of phlogiston, by which the air contiguous to it is diminished; and, as far as I

have had an opportunity of observing, the continuance of the same process, by which the grey calx is converted into red lead, is of the same nature, viz. a farther discharge of phlogiston, and consequently a diminution of the air. For I threw the focus of a burning lens upon a small quantity of the grey calx of lead, placed under a receiver standing in water; and though this operation did not make it absolutely red lead, it gave it a reddish tinge; and I found, by the test of nitrous air, that the air in which the experiment was performed was considerably injured, being not nearly so much affected by nitrous air as common air is. In fact, the calx of lead imbibes dephlogisticated air from the atmosphere, as does the vapour of mercury in the experiment above-mentioned.

Spirit of wine, which affects nitrous air in a manner similar to the effect that oil of turpentine has upon it, affects common air also in a similar manner, and in both cases also in a much less degree. When I agitated a quantity of common air in spirit of wine, it was not much injured, but was sensibly affected, as appeared by its not being quite so much diminished by nitrous air as it would have been before.

Though *ether* be an oil, I do not find that it has any power of parting with its phlogiston to common air, so as sensibly to injure it. To a phial half
filled

filled with water I put a small quantity of ether, and then inverted the phial in a basin of water, so that the air within the phial was exposed to the influence of the ether. Things being thus circumstanced, I observed that, after some time, the air was diminished about one third; but being examined after it had continued a month in this situation, and all the ether had disappeared, the air was not sensibly changed. It was diminished as much as before by nitrous air, and was not in the least degree inflammable. The reason of its apparent diminution could only be this, that the first effect of ether, as I observed a long time ago, is to double the bulk of any kind of air into which it is introduced; but that, in all these cases, part of the increase always disappears, probably by the absorption of part of the vapour of the ether diffused through it.

SECTION VIII.

Of the Power of Oil of Turpentine, both to phlogistate and absorb Air.

I Have observed that *oil of turpentine* has the power not only of diminishing, and phlogistating, but also of actually *absorbing* a very great proportion of common air, in a manner that is very remarkable.

The first time that I exposed common air to the influence of oil of turpentine (which was about the same time that I was trying its effects upon nitrous air, and which I was led to do in consequence of observing its remarkable power of decomposing that kind of air) I found that when I placed a phial of oil of turpentine with its mouth in a basin of the same, leaving about one fourth of the contents of the phial of common air, it was very considerably diminished, and without any agitation, in a few hours, so that it extinguished a candle. It had a still greater effect in diminishing dephlogisticated air. At another time oil of turpentine, applied in the *same* manner, absorbed more than three fourths of a quantity of common air; and some oil of turpentine,

pentine, which had before imbibed a very large quantity of nitrous air, also diminished common air, but not in so great a degree.

My reader will not wonder that I was exceedingly surprized at this power of oil of turpentine to absorb so great a proportion of common air, in the above-mentioned experiment, when he recollects that in no former case, had I ever found that common air was diminished more than *one fourth* by any phlogistic process. But that oil of turpentine would do much more than this, I had repeated proof. I have one memorandum of a quantity of oil of turpentine absorbing five sixths of a quantity of common air. The residuum in this experiment I examined very particularly, and found it to be, in all respects, the same thing that I have denominated *phlogisticated air*; for it did not make lime water turbid, was not affected by nitrous air, and extinguished a candle. At the same time four fifths of a quantity of dephlogisticated air was absorbed by the same fluid, and the remainder extinguished a candle; and, I doubt not, would have appeared, if it had been examined, to have been completely phlogisticated.

After this I was exceedingly puzzled to find that I could not make a quantity of oil of turpentine absorb more than one fourth of the common air to which it was exposed. Even *long time*, and *agitation*,
had

had no more effect; and yet this was of the very same oil of turpentine that I had made use of before. But reflecting on the prodigious difference in these results, it occurred to me that, possibly, the oil of turpentine, in the state in which I first made use of it, might have been deprived of all air; and being, perhaps, capable not only of imparting phlogiston to air, and thereby of diminishing it in the usual proportion, but also of *imbibing* the air in *substance*, it had, in the former case, produced both these effects; whereas having since that time been much exposed to the open air, it had become saturated with it, and therefore could have no effect of imbibing, but only of phlogisticating air, and consequently of diminishing it one fourth, which was the utmost extent of other purely phlogistic processes.

In order to ascertain this, I placed a quantity of the same oil of turpentine under the receiver of an air-pump; and, exhausting it, presently found that it discharged air in great plenty; and immediately after this exposing a quantity of common air to it, in the manner mentioned above, I found that, in the space of a day, one half of it was absorbed.

In order to try whether *other essential oils* had the property of phlogisticating common air, as well as oil of turpentine, I carefully took out the corks of three phials, two of which contained oil of mint, and the other oil of cinnamon; and found that pieces of red-hot

hot wood were instantly extinguished in them all; so that I was fully satisfied that the air was phlogisticated, without conveying it into other phials through water, which would have been attended with the loss of the oils.

Being employed at the same time in making experiments on *caustic alkali*, and other substances, to ascertain their effects on several kinds of air, I placed under the same receiver of the air-pump a quantity of *oil of turpentine*, of *caustic alkali*, and of *distilled water*; and found that they all contained a good deal of air: and then, nearly filling equal phials with each of these fluids, and inverting them in basons of the same, so that equal quantities of common air were left in all the phials, exposed to equal surfaces, and equal quantities of the several fluids; I observed that the oil of turpentine immediately and vigorously absorbed the air, so that in a few hours it had imbibed about one half of the whole quantity exposed to it, while the water had imbibed but very little, and the caustic alkali none at all that I could perceive.

SECTION IX.

Of the Effect of Spirit of Nitre on Air.

I have sufficient proof, that the nitrous acid, both when combined, as it generally is, with water, and likewise when exhibited in the form of vapour, or air, is so loaded with phlogiston, as to be capable of diminishing both the common air, and the nitrous air, which are exposed to it. This I thought to be a fact of a pretty extraordinary kind; it appeared so much so to me, that I had expected the very contrary effect from the experiments that I made upon it; having imagined that, since the nitrous acid constitutes the purest of all the kinds of air, common air wanted nothing more than a greater proportion of this acid to become dephlogistated air; and thus I was in hopes of being master of a process, by which I could not only restore vitiated air to its original purity, but even improve the purity of common air itself.

But when I had got my phials with ground-stoppers, I introduced the tube of one of them, into
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which I had put some strong spirit of nitre, under the edge of a small jar, filled with air that had been injured by putrefaction about a year before; and making it boil, forced the hot fumes to rise into, and mix with it, for a considerable time, till the acid seemed to be almost expelled from it; but I could not perceive that the air was sensibly altered by this operation. It was no more diminished by nitrous air than it had been before.

Then it occurred to me that I had, at that very time, a very good opportunity of ascertaining the effect of the fumes of spirit of nitre on common air, by means of a quantity of strong smoking spirit of nitre, contained in a large phial, one fourth part of which was full, and which had not been opened for half a year; so that all the inclosed air, which was three fourths of the contents of the phial, had been exposed to the vapour of the spirit of nitre all that time.

By losing the spirit of nitre, I might have transferred this air into another vessel; without any mixture of common air; but not chusing to do that, I poured it into another phial, by which means I got a mixture, three-fourths of which was the air from the phial, and one-fourth atmospherical air, for which I had to make an allowance. Examining this mixture by the test of nitrous air, I found that

two

two measures of it, and one of nitrous air, occupied the space of two measures and a half, and a candle would not burn in it; so that the spirit of nitre must, as I then thought, have imparted phlogiston to the air which had been exposed to it, and in so great a degree, that it had become almost perfectly noxious; as may be easily concluded, by allowing for the mixture of common and wholesome air with it in this experiment.

That *acid* fumes, as such, have not this effect upon common air, I, at the same time, ascertained, by making the same experiment on the air of the phial which had contained the strongest spirit of salt; and, I believe, for a longer time. This, however, was in all respects as good as common air.

Afterwards, when I had contrived to fumigate different kinds of air, with the vapour of spirit of nitre, by a singular kind of process which will be mentioned below, I found that it had no effect whatever upon common air; for in this case, I believe it contained very little phlogiston.

I have observed above, that common air is phlogisticated by continuing a considerable time involved in the red vapour of spirit of nitre. This, contrary to my expectation, I also found to be the case with the colourless, or invisible vapour of spirit of nitre, after all the colouring phlogistic matter had been

driven out of it. Air that had continued only two days in a phial with a glass stopper, which contained some of this colourless acid, was sensibly less affected by nitrous air than common air was; and the air that had been confined in the same glass tube in which some of the colourless nitrous acid had been placed in the sand furnace only two days, though the heat had been so small as to have produced no change of colour in the acid, was so much phlogisticated, that one measure of it, and one of nitrous air occupied the space of 1.81 measures.

In order to compare the effects of the same spirit of nitre on common and on dephlogisticated air, I once, for about a fortnight, exposed two half-ounce phials, nearly filled with the strongest brown spirit of nitre, one of them to a quantity of common air, and the other to about an equal quantity of dephlogisticated air; and I observed that the air in each of these cases was diminished in about the same proportion, and that the dephlogisticated air was considerably injured. The surface of the spirit of nitre in each of the phials was become much lighter-coloured than before, and the brown colour had, throughout, intirely disappeared; owing, as I then thought, to its having parted with its phlogiston; and what I thought something remarkable, clouds of some whitish matter, not much unlike flowers of zinc, floated on the
surface

surface of the spirit of nitre in both the phials. The quantity of acid was sensibly diminished in both; and carefully pouring off the upper and lighter-coloured part of the acid in both the phials, I found that they yielded nitrous air in nearly the same proportion, which was as *four* to *nine*, of what they had done before; while the stronger part of the acid, at the bottom of the phials, yielded nitrous air in the proportion of *seven* to *nine* of what they had done before.

I have found air to be diminished by the effluvia of *nitrous ether*. For the air which had been confined about a week, in a bottle in which a quantity of nitrous ether had been kept, was so much injured, that two measures of it, and one of nitrous air, occupied the space of two measures and a half. As I let a good deal of common air into the phial, at the same time that (not chusing to lose it) I poured the ether out of it into another phial, I conclude that the air in the phial was almost perfectly noxious.

I also found that air was injured by being exposed to fresh melted nitre. I had been led to make this experiment by observing that nitre, when it is fused by heat, yields air. Seeing this, I had the curiosity to try whether it would recover the air it had lost by being exposed to the common air, and at the same time to observe what effect this exposure would have upon the common air, in order to judge what
it

it was that nitre, in those circumstances, took from the air. And I find that common air, exposed to nitre in these circumstances, is a little injured, but with such circumstances attending the *proof* of it, as I had never observed before, and which I cannot well account for. The facts were as follow.

I melted about an ounce of nitre in a crucible, and immediately placed it under a receiver standing in water, where it presently became solid. The next morning I examined the air in which it had stood; and found it to be not so good as common air.

Afterwards I melted some nitre in a glass-phial, and the vessel being broken by the expansion of the nitre in cooling, I exposed the nitre to a quantity of air confined by water, so that the common air had access to it on all sides; whereas, in the former experiment, it had been contiguous to it at its surface only. After about a week I examined this air, by the test of nitrous air, and found it to be considerably worse than common air; two measures of it, and one of nitrous air, occupying the space of two measures only: whereas the mixture of common air made at the same time, and with part of the same quantity of nitrous air, was diminished as much as usual. I did not carry this experiment any farther, as I did with another quantity of common air, which had likewise been exposed about a week to melted nitre in the same circumstances,

Two measures of this air, and one of nitrous, at first occupied the space of little more than two measures; but it kept continually approaching to the degree of diminution of a mixture of common air made at the same time, till after four days the difference between them was very small. Whether after a longer time these two mixtures would have been reduced to the very same dimensions, I cannot tell. I made no more experiments of this kind, nor have I, in any other respect, pursued this singular kind of fact any farther.

I observed that the vapour of marine acid does not injure air, and I had the fairest opportunity of trying this; when I had exposed a quantity of this acid, in a glass tube hermetically sealed, during several months, to a sand heat. For the air within this tube, being examined several months after it had been exposed to that heat, was found to be not at all injured. The air which had been confined along with vinegar in the same manner, and the same space of time, was so far injured, that, with an equal quantity of nitrous air, the measures of the test were 1.44.

SECTION X.

Air injured by the Effluvium of Water fresh distilled.

NOTWITHSTANDING it has been a maxim with chemists, that water contracts no union with phlogiston, it is acknowledged that water fresh distilled acquires something of an empyreumatic nature, which gives it an unpleasant flavour, and which goes off by exposure to the open air. That this volatile principle is phlogiston I thought that I had ascertained by exposing air to the influence of it.

I took water fresh distilled in copper, and filled a phial about half full of it, and examining the air within the phial about a month afterwards, I found it so much phlogisticated, that one measure of it and one of nitrous air occupied the space of 1.32 measures; when, with the same nitrous air and common air, the same measures were 1.22.

In this case it might be suspected that the phlogiston came from the copper. But at the same time I made a similar experiment, with a similar result, on water distilled in glass. In this case there was more air, and a smaller quantity of water in the phial, but the time of exposure was nearly the same;

and with this air the measures of the test were 1.26. It is probable that with more water, more time, and less air, the result would have been more considerably in favour of the water having acquired phlogiston in the act of evaporation, without any communication with substances that are thought to contain it. This experiment, however, is sufficiently similar to the others I have recited, in which mere *heat* had the same effect as the communication of phlogiston. However, the true explanation of these appearances is, that water fresh distilled, is disposed to imbibe pure air, which, accordingly, may be expelled from water that has been long exposed to it.

To these observations on the phlogification, as I have called it, of air by means of water, I shall add others in which phlogified air was, in some degree, purified by agitation in it; which, though I did not suspect it at the time, must have been owing to purer air from the water, mixing with the more impure air agitated in it.

Having rendered inflammable air perfectly innocuous by continued *agitation in a trough of water*, deprived of its air, I concluded that other kinds of noxious air might be restored by the same means; and I presently found that this was the case with putrid air, even of more than a year's standing. I shall observe once for all, that this process has never failed to restore any kind of noxious air on which

which I have tried it, viz. air injured by respiration or putrefaction, air infected with the fumes of burning charcoal, and of calcined metals, air in which a mixture of iron filings and sulphur, or that in which paint made of white lead and oil, has stood, or air which has been diminished by a mixture of nitrous air. Of the remarkable effect which this process has on nitrous air itself, an account has been given in its proper place.

If this process be made in water deprived of air, either by the air-pump, by boiling, or by distillation, or if fresh rain-water be used, the air will always be diminished by the agitation; and this is certainly the fairest method of making the experiment. If the water be fresh pump-water, there will always be an increase of the air by agitation, the air contained in the water being set loose, and joining that which is in the jar. In this case, also, the air has never failed to be restored; but then it might be suspected that the melioration was produced by the addition of some more wholesome ingredient. As these agitations were made in jars with wide mouths, and in a trough which had a large surface exposed to the common air, I take it for granted that the noxious effluvia, whatever they be, were first imbibed by the water, and thereby transmitted to the common atmosphere. In some cases this was sufficiently indi-

rated by the disagreeable smell which attended the operation.

I shall conclude this section with observing, that I have found a remarkable difference in different kinds of water, with respect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitated common air in the water of a deep well, near my house in Calne, which is hard, but clear and sweet, a candle would not burn in it after three minutes. The same was the case with the rain-water, which I got from the roof of my house. But in distilled water, or the water of a spring-well near the house, I agitated the air about twenty minutes, before it would be so much injured. It may be worth while to make farther experiments with respect to this property of water.

SEC.

SECTION XI.

Of the Effluvia of Flowers in Air.

THE action of a plant considered as simply *vegetating* in air is a thing quite different from the effect that the *exhalation of the flowers*, and perhaps other particular parts of the plant, may have upon it. *Smell*, the old chemists said, was an indication of phlogiston, and I find that the most delicate flowers injure the air much more than I had imagined. Nothing is sweeter than a *rose*, and yet the fragrant effluvia of it is far from being favourable to the air in which it is confined.

On the 25th of June I confined a full blown red rose in about four ounce measures of common air, having covered it with a small glass jar standing in water; and I observed that, the next day, the air was so much injured, that one measure of it, and another of nitrous air occupied the space of 1.75 measures; so that I doubt not that any animal would have expired immediately on being put into it. The day following the measures of the test were 1.9, and

and the day after something more. Notwithstanding this, when the rose was withdrawn, it did not seem to have lost any thing of its agreeable fragrance.

SECTION XII.

Of the Effect of the electric Spark on Air.

AIR is diminished and rendered noxious by the *electric spark*, though I had no expectation of this event when I made the experiment.

The experiments which I made with electricity were solely intended to ascertain what has often been attempted, but, as far as I know, had never been fully accomplished, viz. to change the blue colour of liquors, tinged with vegetable juices, red.

For this purpose I made use of a glass tube, about one tenth of an inch diameter in the inside, as in Pl. II. fig. 16. In one end of this I cemented a piece of wire *b*, on which I put a brass ball. The lower part from *a* was filled with water tinged blue, or rather purple, with the juice of turnsole,

or

or archil. This is easily done by an air-pump, the tube being set in a vessel of the tinged water.

Things being thus prepared, I perceived that, after I had taken the electric spark, between the wire *b*, and the liquor at *a*, about a minute, the upper part of it began to look red, and in about two minutes it was very manifestly so; and the red part, which was about a quarter of an inch in length, did not readily mix with the rest of the liquor. I observed also, that if the tube lay inclined while I took the sparks, the redness extended twice as far on the lower side as on the upper.

The most important, though the least expected observation, however, was that, in proportion as the liquor became red, it advanced nearer to the wire, so that the space of air in which the sparks were taken was diminished; and at length I found that the diminution was about one fifth of the whole space; after which more electrifying produced no sensible effect.

To determine whether the cause of the change of colour was in the *air*, or in the *electric matter*, I expanded the air which had been diminished in the tube by means of an air-pump, till it expelled all the liquor, and admitted fresh blue liquor into its place; but after that, electricity produced no sensible effect, either on the air, or on the liquor;
so

so that, as I then thought, it was evident, that the electric matter had decomposed the air, and had made it deposit something that was of an acid nature. But Mr. Cavendish has proved that nitrous acid was formed in this process by an union of the dephlogisticated and phlogisticated air in common air.

In order to determine whether the *wire* had contributed any thing to this effect, I used wires of different metals, iron, copper, brass, and silver; but the result was the very same with them all.

It was also the same when, by means of a bent glass tube, I made the electric spark without any wire at all, in the following manner. Each leg of the tube, Pl. II. fig. 19, stood in a basin of quicksilver; which, by means of an air-pump, was made to ascend as high as *a, a*, in each leg, while the space between *a* and *b* in each contained the blue liquor, and the space between *b* and *b* contained common air. Things being thus disposed, I made the electric spark perform the circuit from one leg to the other, passing from the liquor in one leg of the tube to the liquor in the other leg, through the space of air. The effect was, that the liquor, in both the legs, became red, and the space of air between them was contracted, as before.

Air

Air thus diminished by electricity makes no effervescence with, and is no farther diminished by, a mixture of nitrous air; so that it must have been in the highest degree noxious, exactly like air diminished by any other process.

I took the electric spark in common air confined by quicksilver; and then, admitting to it water tinged blue with the juice of turnsole, it became red in the space of a day and two nights, but the colour did not change presently. Also, after this the diminution was greater than it had been before.

Having taken the electric spark in common air upon quicksilver, as before, it was presently diminished as usual; and the next day without any farther electrification, the diminution was more considerable. The third day I admitted to it the juice of turnsole, and in about an hour it appeared to be red at the top, but was not sensibly diminished more than before. In less than a day it became wholly red, and then no farther diminution was apparent.

I took a quantity of water which had been made blue with the juice of turnsole, and which had been made seemingly red with the electric spark, taken in the common air over it; but, on mixing all the parts of it together, it resumed its blue colour
 (the

(the blue colouring matter having only subsided to the bottom) so that alteration in the constitution of this liquor by this process, though manifest to the eye, is not, in fact, so very considerable. It is evident, however, from the preceding observations, that it could not be the mere *concussion* given to the air by the spark, or shock, that had this effect upon it; because when the air was completely diminished, the spark or shock had no effect, and the liquor turned red when it was admitted to the air a long time after the operation of the electric spark upon it, while it was confined by quick-silver.

S E C.

SECTION XIII.

Of the Effect of putrid Marshes on Air.

TO these sections on the subject of diminished, and noxious air, or as it might have been called *phlogisticated air*, I shall subjoin a letter which I addressed to Sir John Pringle, on the noxious quality of the effluvia of putrid marshes, and which was read at a meeting of the Royal Society, December 16, 1773.

This letter, which is printed in the Philosophical Transactions, vol. 74, p. 90, is immediately followed by another paper, to which I would refer my reader. It was written by Dr. Price, who has so greatly distinguished himself, and done such eminent service to his country, and to mankind, by his calculations relating to the probabilities of human life, and was suggested by his hearing this letter read at the Royal Society. It contains a confirmation of my observations on the noxious effects of stagnant waters, by deductions from Mr. Muret's account of the Bills of Mortality for a parish situated among marshes, in the district of Vaud, belonging to the Canton of Bern in Switzerland.

To

To Sir JOHN PRINGLE, Baronet.

DEAR SIR,

Having pursued my experiments on different kinds of air considerably farther, in several respects, than I had done, when I presented the last account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleased to take of them, I shall continue my communications on this subject. But, without waiting for the result of a variety of processes, which I have now going on, or of other experiments, which I propose to make, I shall, from time to time, communicate such detached articles, as I shall have given the most attention to, and with respect to which, I shall have been the most successful in my enquiries.

Since the publication of my papers, I have read two treatises, written by Dr. Alexander, of Edinburgh, and am exceedingly pleased with the spirit of philosophical inquiry which they discover. They appear to me to contain many new, curious, and valuable observations; but one of the *conclusions*, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and
from

from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes.

I was particularly surpris'd, to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise *on the diseases of the army*. On this account, I have thought it not improper to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. Alexander's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. Alexander has endeavoured to prove, will preserve other substances from putrefaction; because, being already saturated with the putrid effluvia, it cannot readily take any more; but Dr. Alexander was not aware, that air thus loaded with putrid effluvia is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water so near at hand as I had there, I neglected to change

it, till it turned black, and became offensive, but by no means to such a degree, as to deter me from making use of it. In this state of the water, I observed bubbles of air to rise from it, and especially in one place, to which some shelves, that I had in it, directed them; and having set an inverted glass vessel to catch them, in a few days I collected a considerable quantity of this air, which issued spontaneously from the putrid water; and putting nitrous air to it, I found that no change of colour or diminution ensued, so that it must have been, in the highest degree, noxious. I repeated the same experiment several times afterwards, and always with the same result.

After this, I had the curiosity to try how wholesome air would be affected by this water; when, to my real surprise, I found, that after only one minute's agitation in it, a candle would not burn in it; and, after three or four minutes, it was in the same state with the air, which had issued spontaneously from the same water.

I also found that common air, confined in a glass vessel, in *contact* only with this water, and without any agitation, would not admit a candle to burn in it after two days.

These facts certainly demonstrate, that air which either arises from stagnant and putrid water, or which has been for some time in contact with it, must

must be very unfit for respiration; and yet Dr. Alexander's opinion is rendered so plausible by his experiments, that it is very possible that many persons may be rendered secure, and thoughtless of danger, in a situation in which they must necessarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. Alexander appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

I am, &c.

In all the preceding experiments, there is a remarkable *diminution* of common, or respirable air, in proportion to which it is always rendered unfit for respiration, indisposed to effervesce with nitrous air, and incapable of farther diminution from any other cause.

All these processes, I observed, agree in this one circumstance, and I believe in no other, that the principle which the chemists call *phlogiston* is set loose; and therefore I concluded that the diminution of the air was, in some way or other, the consequence of the air becoming overcharged with phlogiston, and that water, and growing vegetables, tend to restore this air to a state fit for respiration, by imbibing the superfluous phlogiston.

For this reason I said, in the first account of these experiments, if it was thought convenient to intro-

duce a new term (or rather make a new application of a term already in use among chemists) it might not be amiss to call air that has been diminished, and made noxious by any of the processes above mentioned, or others similar to them, by the common appellation of *phlogisticated air*.

Mr. Lavoisier, however, has proved that in all these *phlogestic processes*, as I termed them, of air, the purer, or dephlogisticated part of it, is absorbed, though he does not admit that any thing is emitted. In what I shall advance on the subject of phlogiston, reasons will be given for concluding that both those things take place at the same, dephlogisticated air uniting with the phlogiston emitted from the substance to which the air is exposed, and forming with it either nitrous acid, or fixed air.

PART

P A R T III.

MISCELLANEOUS OBSERVATIONS RELATING TO
PHLOGISTICATED AIR.

SECTION I.

1. *Of the Purity of Air in different Circumstances.*

WHEN I first discovered the property of nitrous air as a test of the wholesomeness of common air, I flattered myself that it might be of considerable practical use, and particularly that the air of distant places and countries might be brought and examined together, with great ease and satisfaction; but I own that hitherto I have rather been disappointed in my expectations from it. My own observations have not, indeed, been many; but according to them the difference of the open air in different places, as indicated by a mixture of nitrous air, is generally inconsiderable; and

I have reason to think that when very unwholesome air is conveyed to a great distance, and much time elapses before it is tried, it approaches, by some means or other, to the state of wholesome air. At least such I found to be the worst air that has at any time been sent to me into Wiltshire from distant manufacturing towns and workshops, &c. in them, where the air was thought to be peculiarly unwholesome. I am satisfied, however, from my own observations, that air may be very offensive to the nostrils, probably hurtful to the lungs (and perhaps also in consequence of the presence of phlogistic matter in it) without the phlogiston being so far *incorporated with it*, as to be discoverable by the mixture of nitrous air.

I gave several of my friends the trouble to send me air from distant places, especially, from manufacturing towns, and the worst they could find to be actually breathed by the manufacturers, such as is known to be exceedingly offensive to those who visit them; but when I examined those specimens of air in Wiltshire, the difference between them and the very best air in this county, which is esteemed to be very good, as also the difference between them and specimens of the best air in the counties in which those manufacturing towns are situated, was very trifling.

Mr.

Mr. Boulton of Birmingham was so obliging as to send me a great variety of specimens of air from that manufacturing town, along with an account of his own examination of them by the test of nitrous air. I shall only note his account of four of the specimens, including the best and the worst, and reducing his numbers to my own.

	Measures.
The air in a garden near the new church	1.39
The bottom of the old church steps, very low and close	1.45
The middle of Mr. Taylor's manufactory	
The horn button manufactory	

When I examined them myself, on the 12th of December, 1777, the former was as nearly as possible the same with the air of pretty high ground in Wiltshire; so that the difference between the worst air in the manufactories at Birmingham, and very good air was .06. On the 3d of July following, I examined the remainder of the same specimens of air again, and found the difference between them and good air to be .02; and at the end of October it was only .01.

Dr. Percival also was so good as to send me several specimens of air from Manchester, and one from his country house at Hart-hill, about three miles from Manchester, the highest and healthiest situation in that part of the country. The air of

this place was nearly the same with that of Wiltshire; and when I examined the specimens he sent me on the 3d of July, 1778, the measures of the test for this air were 1.27, of the air from a weaving shop in Manchester 1.305, and of the market place 1.295. The difference therefore between the former and pure air was only .035, and of the latter only .025.

The worst air that I have yet found breathed by men, and that was sent from a distance, was from a coal-pit in the neighbourhood of Bristol. For the difference between good air and that which was taken in the shaft of the pit ten yards below the mouth was .07, and between the same and that which was taken where the men were at work was .21.

Mr. William Vaughan took the trouble to procure me a specimen of air from a calico printing house, which was exceedingly offensive, and I have no doubt of its having been taken very properly, and having been well secured from all communication with the external air; and yet when I examined it in Wiltshire the difference between it and good common air was only .02.

Mr. S. Vaughan, senior, on his passage from Jamaica, brought me two bottles of air, one from the hold of the ship, intolerably offensive, the other the fresh air above deck about 30' N. but the difference

ference between these specimens of air, and the air of Wiltshire, was quite inconsiderable.

I have frequently taken the open air in the most exposed places in this country at *different times of the year*, and in different states of the *weather*, &c. but never found the difference so great, as the inaccuracy arising from the method of making the trial might easily amount to, or exceed,

2. *Of the State of the Air in Hot-houses,*

There is generally a sense of oppression, or difficult respiration, felt on entering a *hot-house*, which seems to proceed from something different from mere heat; for we feel nothing of that sensation in an equally warm, well aired room; but my observations on this kind of air would not have indicated any such thing. On the 3d of June, 1778, I took the air in three several hot-houses adjoining to each other, but having different degrees of heat, and found that one measure of that air and one of nitrous air occupied the space of 1.29 measures; when the result of the same experiment with the external air, taken at the same time, was 1.27, a difference certainly very inconsiderable.

3. *Of the State of the Air in Dining-Rooms.*

Large and lofty rooms are generally preferable to small and low ones. But this is only the case when the same company confine themselves in it the same space of time, with the doors, &c. shut; for, having more air to breathe, it will certainly require more time to contaminate it. But when the company is large, or processes are going on that will effectually contaminate the air (as many candles burning in the room, hot victuals, continuing a long time upon the table, &c.) a small room is much preferable, unless there be an opening in the top of the large room, that will easily promote a change of air in it. Because the occasional opening of the door in a small room will generally produce a sufficient change of a great part of the air; whereas the height of the door bearing but a small proportion to the height of a large and well proportioned dining room, the opening of the door, or even its continuing open, has very little effect. The extreme offensiveness of the air in these circumstances is not perceived by persons who sit in it from the beginning, but it is immediately perceived by persons who step out of the room, and return to it.

Dining one time in a company of not more than eight or ten persons, in a large and very lofty room; and being called out presently after the cloth was removed, I was much struck with the offensiveness of the air on my return; and being willing to ascertain the *degree* in which it was injured, I took occasion on some pretence or other, to pour the water from one full decanter into another, and putting in the stopper, saw that nobody opened it till the company separated. I then took the decanter into my laboratory, and examined the air at my leisure; when it appeared to be much contaminated. For one measure of this air, and one of nitrous air, occupied the space of one 1.31 measures; when the same experiment being made with the air of a well ventulated room in the same house, the measures were 1.25. At the same time I breathed a quantity of air till it just extinguished a candle, and found that the measures were 1.43. So that, had the air of the dining room received a little more than twice as much more phlogistic matter, as it was charged with by the breathing of these eight or ten persons, the effluvia of the victuals, &c. a candle would not have burned in the room. I would advise, therefore, that when such large dining rooms are built, provision be made for letting out the vitiated air at the top of them. For breathing such contaminated air so long a time as it is now
the

the custom to do, at and after dinner, must be very hurtful. Otherwise if it were not inconvenient on other accounts, it would be better to have the dinner in one room, and the desert in another.

SECTION II.

Of the Manner in which Air is phlogisticated by Means of inflammable Air.

HAVING been led by various experiments to expect, and even to believe, that common air is usually phlogisticated by actually decomposing a small quantity of inflammable air, admitted to it in its nascent state (notwithstanding large quantities of inflammable air ready formed have no sensible effect upon it) I wished to ascertain so extraordinary a fact, by some experiments of a more decisive nature, and with that view I made the following.

First I took a pot of iron filings and sulphur, which I had found to have been in a state of yielding inflammable air in water about three months, and
which

which I therefore presumed would continue for some time in the same state. This pot being introduced to a quantity of common air made no addition to it, but diminished it, and phlogisticated it as usual.

I then took a quantity of this mixture, which had yielded inflammable air many months, in a vessel of water. On the 22d of September I introduced some common air into the vessel in which it was contained, and on the 26th of October I observed that, though this mixture, now covered with water, had thrown up bubbles of air, which mixed with the common air on the surface of the water, that air was sensibly diminished, though not more than one tenth in all, and being examined was found to be phlogisticated, and to have nothing inflammable in it. At the same time a quantity of dephlogisticated air, exposed in a similar manner, was diminished from eleven and a half to one third; and from having been very pure, the measures of the test, with two equal quantities of nitrous air, were now 1.24, A candle burned in it better than in common air, but there was nothing inflammable in it.

But the most decisive experiment that I made of this kind was the following. A quantity of iron filings and sulphur were mixed, and put into a phial filled up with water, on the 24th of June 1779, and on the 25th of July following it had yielded a quantity of inflammable air, which was then all taken out;
and

and on the 22d of September more inflammable air was produced, about two ounce measures in all. Having by this means satisfied myself that this mixture was in a state of yielding inflammable air, I introduced to it a quantity of common air (the phial having been always kept inverted in a basin of water) and on the 26th of October I found the common air very considerably diminished. This air then being thrown out, the mixture was kept in the phial, now filled with water. In these circumstances, it continued to yield air, and when an ounce measure and a half was produced, which was on the 24th of March 1780, I examined it, and found it to be strongly inflammable. There could therefore be no doubt but that the common air in this experiment had been diminished and phlogisticated by an addition of inflammable air in its nascent state, or rather after it was completely, though but *newly formed*.

I do not by any means infer that, because common air was diminished in this case, in consequence of an accession of inflammable air in its nascent state, that it is never diminished in any other manner; but perhaps it will be found that all the substances which we know to phlogisticate common air are likewise capable of yielding inflammable air, if not in the temperature of the atmosphere, at least by means of *heat*, or in some other process. This I found to be the case with metals,
and

and it has been seen to be so, in one instance, with liver of sulphur, and it is remarkably the case with all putrefactive substances. Vaults containing human excrements are often found to abound with inflammable air, and they, like other putrefactive substances, diminish common air.

The preceding experiments on the phlogistication of common air by means of inflammable air, led me to try whether a degree of heat short of ignition would not make a quantity of inflammable air, ready formed, part with its phlogiston to common air. For this purpose I mixed two thirds of common air with one third of inflammable air, and I kept them as hot as I could do without melting the glass vessel in which they were contained several hours; but afterwards they occupied the same dimensions as before. This air was confined in a glass jar, the upper part of which was surrounded with hot coals, by means of the instrument Pl. III. fig. 4.

I then tried what *length of time* would effect in this case; but though I always found a very sensible, and sometimes a considerable diminution, the phlogistication was never completed, and the progress of it always stopped, without ever proceeding farther in any length of time afterwards. The following facts in proof of this it may be just worth while to recite.

A mixture

A mixture of one third inflammable air and two thirds common air, which had been confined by water from the 30th of June 1779, was diminished the 18th of March 1780 one twelfth, and burned with a lambent blue flame.

A mixture of one third inflammable air and two thirds common air, from some time in the month of June 1779, was diminished one thirteenth on the 20th of July 1780; but it had been diminished nearly as much on the 5th of October preceding. It burned with a considerable explosion.

Another quantity, one third inflammable air from marshes, and two thirds common air, from the same time, was diminished one tenth; but on the 18th of March preceding it was diminished one twelfth, and then burned with a blue flame. On the 20th of July 1780, it burned in the very same manner.

Exactly similar to these were the experiments that I made with various mixtures of dephlogisticated air and inflammable air. Air one half inflammable and one half dephlogisticated, mixed in June 1779, was on the 20th of July 1780 diminished one fifth; but it had been diminished two thirds of this the 4th of October preceding. It burned with a considerable explosion.

Another mixture of the same kind, and made at the same time, was, on the 3d of July 1779, diminished

nished two elevenths, and on the first of January 1780, it was diminished two fifteenths more. On the 20th of July 1780, it was in the very same state, and burned with a very considerable explosion.

SECTION III.

Of the Effect of Water on phlogisticated Air.

IN the infancy of my experiments I concluded, that all kinds of air were brought by agitation in water to the same state, the purest air being partially phlogisticated, and air completely phlogisticated being thereby made purer; inflammable air also losing its inflammability, and all of them being brought into such a state, as that a candle would just go out in them. This inference I made from all the kinds of air with which I was then acquainted, and which did not require to be confined by mercury, being brought to that state by agitation in a trough of water, the surface of which was exposed to the open air; never imagining that when the air

in my jar was separated from the common air by a body of water, generally about twelve inches in depth (adding that within to that without the jar) they could have any influence on each other. I have, however, been long convinced that, improbable as it then appeared to me, this is actually the case, though a fact so remarkable well deserves to be farther attended to.

As I have no where recited any experiments which prove that this change in the state of the air, agitated in water, depends upon that water having some communication with the atmosphere, I shall do it here, after an account of some other experiments, from which it is equally evident that, notwithstanding every possible precaution, the air will be changed if there be that communication.

I agitated, a quarter of an hour, about three ounce measures of air, phlogisticated with nitrous air, in a vessel containing twenty ounces of water, which had been boiled several hours, and while it was very warm; after which it was diminished one sixth, and was so much improved, that the standard of it was 1.74. The next day I agitated the remainder of it another quarter of an hour in the water which had been boiled at the same time, when it was diminished about one tenth, and the standard of it was 1.57.

I also

I also agitated the same quantity of air, phlogisticated with iron filings and sulphur, in the same manner twenty minutes, when it was diminished one seventh, and the standard of it was 1.66.

I afterwards expelled a little air from the water in which this air had been agitated; and from that with which the first of these experiments was made, I got air, the standard of which was 1.66; and from the water in which the second experiment was made, air of the standard of 1.33. In this a candle would have burned. It is evident, from the diminution of the air in these experiments, that part of it is absorbed by the water. But it is no less evident, as will appear by the next experiment, that the place of some part of the air which had been absorbed was supplied by a purer air, which the water had imbibed from the atmosphere. That water does imbibe the purer part of atmospherical air, in preference to that which is impure, is evident from the air that is expelled from it by heat. For if the water be clear, and free from any thing that is putrescent, the air expelled from it is generally of the standard of 1.0; whereas, that of the atmosphere, when the nitrous air is the purest, is about 1.2.

But the experiment which decisively proved that my former conclusion was fallacious was the following. After expelling all air, by boiling, from a

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quantity

quantity of water, I put to it, in separate phials, air that had been phlogisticated with iron filings and sulphur; and corking them very well, I left them with their mouths in water, and occasionally agitated them. After about two months I examined them, and I found both the air that was confined by the water, and also that air which by heat I expelled from the water, completely phlogisticated. The same was the result of a similar experiment, in which the air had been expelled from the water by the air-pump.

BOOK

B O O K VI.

OF SOME KINDS OF AIR THAT ARE READILY ABSORBED BY WATER.

P A R T I.

OF MARINE ACID AIR.

S E C T I O N I.

The History of the Discovery of this Kind of Air.

BEING very much struck with the result of an experiment of Mr. Cavendish's, related Phil. Trans. Vol. LVI. p. 157, by which, though, he says, he was not able to get any inflammable air from copper, by means of spirit of salt, he got a much more remarkable kind of air, viz. one that lost its elasticity by coming into contact with water, I was exceedingly desirous of making myself ac-

quainted with it. On this account, I began with making the experiment in quicksilver, which I never failed to do in any case in which I suspected that air might either be absorbed by water, or be in any other manner affected by it; and by this means I presently got a much more distinct idea of the nature and effects of this curious solution.

Having put some copper filings into a small phial, with a quantity of spirit of salt; and making the air (which was generated in great plenty, on the application of heat) ascend into a tall glass vessel full of quicksilver, and standing in quicksilver, the whole produce continued a considerable time without any change of dimensions. I then introduced a quantity of water to it; when about three fourths of it (the whole being about four ounce measures) presently, but gradually, disappeared, the quicksilver rising in the vessel.

Having frequently continued this process a long time after the admission of the water, I was much amused with observing the large bubbles of the newly generated air, which came through the quicksilver, the sudden diminution of them when they came to the water, and the very small bubbles which went through the water. They made, however, a continual, though slow, increase of inflammable air.

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The solution of *lead* in the marine acid is attended with the very same phenomena as the solution of copper in the same acid; about three fourths of the generated air disappearing on the admission of water; and the remainder being inflammable.

I had always thought it something extraordinary that a species of air should *lose its elasticity* by the mere *contact* of any thing, and from the first suspected that it must have been *imbibed* by the water that was admitted to it; but so very great a quantity of this air disappeared upon the admission of a very small quantity of water, that at first I could not help concluding that appearances favoured the former hypothesis. I found, however, that when I admitted a much smaller quantity of water, confined in a narrow glass tube, a part only of the air disappeared, and that very slowly, and that more of it vanished upon the admission of more water. This observation put it beyond a doubt, that this air was properly *imbibed* by the water, which, being once fully saturated with it, was not capable of receiving any more.

The water thus impregnated tasted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even dissolved iron very fast, and generated inflammable air. This last observation, together

with another which immediately follows, led me to the discovery of the true nature of this remarkable kind of air.

Happening, at one time, to use a good deal of copper and a small quantity of spirit of salt, in the generation of this kind of air, I was surprized to find that air was produced long after, I could not but think that the acid must have been saturated with the metal; and I also found that the proportion of inflammable air to that which was absorbed by the water continually diminished, till, instead of being one fourth of the whole, as I had first observed, it was not so much as one twentieth. Upon this, I concluded that this subtle air did not arise from the copper, but from the spirit of salt; and presently making the experiment with the acid only, without any copper, or metal of any kind, this air was immediately produced in as great plenty as before; so that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature, that they are not liable to be condensed by cold, like the vapour of water, and other fluids, and therefore may be very properly called an *acid air*, or more restrictively, the *marine acid air*.

When this air is all expelled from any quantity of spirit of salt, which is easily perceived by the subsequent

requent vapour being condensed by cold, the remainder is a very weak acid, barely capable of dissolving iron.

In my first experiments on this species of air, I procured it from spirit of salt, but I afterwards, hit upon a much less expensive method of getting it, by having recourse to the process by which the spirit of salt is itself originally made. For this purpose I fill a small phial with common salt, pour upon it a small quantity of concentrated oil of vitriol, and receive the fumes emitted by it in a vessel previously filled with quicksilver, and standing in a basin of quicksilver, in which it appears in the form of a perfectly *transparent air*, being precisely the same thing with that which I had before expelled from the spirit of salt.

This method of procuring acid air is the more convenient, as a phial, once prepared in this manner, will suffice, for common experiments, many weeks; especially if a little more oil of vitriol be occasionally put to it. It only requires a little more heat at the last than at the first. Indeed, at the first, the heat of a person's hand will often be sufficient to make it throw out the vapour. In warm weather it will even keep smoking many days without the application of any other heat.

On this account it should be placed where there are no instruments, or any thing of metal, that can be corroded by this acid vapour. It is from dear-

bought experience that I give this advice. It may easily be perceived when this phial is throwing out this acid vapour, as it always appears, in the open air, in the form of a light cloud; owing, I suppose, to the acid attracting to itself, and uniting with, the moisture that is in the common atmosphere.

SECTION II.

The Effect of Marine Acid Air on Substances containing Phlogiston.

BEING now in the possession of a new subject of experiments, viz. an elastic acid vapour, in the form of a permanent air, easily procured, and effectually confined by glass and quicksilver, with which it did not seem to have any affinity; I immediately began to introduce a variety of substances to it, in order to ascertain its peculiar properties and affinities, and also the properties of those other bodies with respect to it.

Iron filings, being admitted to this air, were dissolved by it pretty fast; half of the air, disappearing, and the other half becoming inflammable air, not absorbed

absorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many substances to this air, before I discovered that by means of it, inflammable air was produced from most substances that contain phlogiston.

Inflammable air was produced, when to this acid air I put spirit of wine, oil of olives, oil of turpentine, charcoal, phosphorus, bees-wax, and even sulphur. This last observation, I own, surprized me; for, the marine acid being reckoned the weakest of the three mineral acids, I did not think that it had been capable of dislodging the oil of vitriol from this substance; but I found that it had the very same effect both upon alum and nitre; the vitriolic acid in the former case, and the nitrous in the latter, giving place to the stronger vapour of spirit of salt.

The rust of iron, and the precipitate of nitrous air made from copper, also imbibed this air very fast, and the little that remained of it was inflammable air; which proves, that these calces contain phlogiston.

As some remarkable circumstances attend the absorption of this acid air, by the substances above-mentioned, I shall briefly mention them.

Spirit of wine absorbs this air as readily as water itself, and is increased in bulk by that means. Also, when

when it is saturated, it dissolves iron with as much rapidity, and still continues inflammable.

Oil of olives absorbs this air very slowly, and at the same time, it turns almost black, and becomes glutinous. It is also less miscible with water, and acquires a very disagreeable smell. By continuing upon the surface of the water, it became white, and its offensive smell went off in a few days.

Oil of turpentine absorbed this air very fast, turning brown, and almost black. No inflammable air was formed, till I raised more of the acid air than the oil was able to absorb, and let it stand a considerable time; and still the air was but weakly inflammable. The same was the case with the oil of olives, in the last-mentioned experiment; and it seems to be probable, that, the longer this acid air had continued in contact with the oil, the more phlogiston it would have extracted from it.

Bees-wax absorbed this air very slowly. About the bigness of a hazel nut of the wax being put to three ounce measures of the acid air, the air was diminished one half in two days, and, upon the admission of water, half of the remainder also disappeared. This air was strongly inflammable.

Charcoal absorbed this air very fast. About one fourth of it was rendered immiscible in water, and was but weakly inflammable.

A small

A small bit of *phosphorus*, perhaps about half a grain, smoked, and gave light in the acid air, just as it would have done in common air confined. It was not sensibly wasted after continuing about twelve hours in that state, and the bulk of the air was very little diminished. Water being admitted to it absorbed it as before, except about one fifth of the whole. It was but weakly inflammable.

Putting several pieces of *sulphur* to this air, it was absorbed but slowly. In about twenty four hours about one fifth of the quantity had disappeared; and water being admitted to the remainder, very little more was absorbed. The remainder was inflammable, and burned with a blue flame.

Notwithstanding the affinity which this acid air appears to have with phlogiston, it did not at first appear that it was capable of depriving all bodies of it. I found that dry wood, crusts of bread, and raw flesh, very readily imbibed this air, but did not part with any of their phlogiston to it. All these substances turned very brown, after they had been some time exposed to this air, and tasted very strongly of the acid when they were taken out; but the flesh, when washed in water, became very white, and the fibres easily separated from one another,

other, even more than they would have done if it had been boiled or roasted.

But I afterwards observed that, by giving it more time, it will extract phlogiston from substances from which I at first concluded that it was not able to do it, as from dry wood, crusts of bread not burned, and dry flesh. As there was something peculiar to itself in the process or result of each of these experiments, it may not be improper to mention them distinctly.

Pieces of dry *cork wood* being put to the acid air, a small quantity remained not imbibed by water, and was inflammable.

Very dry pieces of *oak*, being exposed to this air a day and a night, after imbibing a considerable quantity of it, produced air which was inflammable indeed, but in the slightest degree imaginable. It seemed to be very nearly in the state of common air.

A piece of *ivory* imbibed the acid vapour very slowly. In a day and a night, however, about half an ounce measure of permanent air was produced, and it was pretty strongly inflammable. The ivory was not discoloured, but was rendered superficially soft and clammy, tasting very acid.

Pieces of *beef*, roasted, and made quite dry, but not burned, absorbed the acid vapour slowly; and
when

when it had continued in this situation all night, from five ounce measures of the air, half a measure was permanent, and pretty strongly inflammable. This experiment succeeded a second time exactly in the same manner; but when I used pieces of white dry *chicken flesh*, though I allowed the same time, and in other respects the process seemed to go on in the same manner, I could not perceive that any part of the remaining air was inflammable.

That inflammable air is produced from *charcoal* in acid air I observed before. I afterwards found that it may likewise be procured from *pit-coal*, without being charred.

Inflammable air I had also observed to arise from the exposure of spirit of wine, and various *oily* substances, to the vapour of spirit of salt. I have since made others of a similar nature, and as peculiar circumstances attended some of these experiments, I shall recite them more at large.

Essential oil of mint absorbed this air pretty fast, and presently became of a deep brown colour. When it was taken out of this air it was of the consistence of treacle, and sunk in water, smelling differently from what it did before; but still the smell of the mint was predominant. Very little or none of the air was fixed, so as to become inflammable;

mable; but more time would probably have produced this effect.

Oil of turpentine was also much thickened, and became of a deep brown colour, by being saturated with acid air.

Ether absorbed acid air very fast, and became first of a turbid white, and then of a yellow and brown colour. In one night a considerable quantity of permanent air was produced, and it was strongly inflammable.

Having, at one time, fully saturated a quantity of ether with acid air, I admitted bubbles of common air to it, through the quicksilver, by which it was confined, and observed that white fumes were made in it, at the entrance of every bubble, for a considerable time.

At another time, having fully saturated a small quantity of ether with acid air, and having left the phial in which it was contained nearly full of the air, and inverted, it was by some accident overturned; when, instantly, the whole room was filled with a visible fume, like a white cloud, which had very much the smell of ether, but peculiarly offensive. Opening the door and window of the room, this light cloud filled a long passage, and another room. In the mean time the ether was seemingly all vanished, but some time after the surface of the quick-

quicksilver in which the experiment had been made was covered with a liquor that tasted very acid; arising, probably, from the moisture in the atmosphere attracted by the acid vapour with which the ether had been impregnated.

This visible cloud I attribute to the union of the moisture in the atmosphere with the compound of the acid air and ether. I have since saturated other quantities of ether with acid air, and found it to be exceedingly volatile, and inflammable. Its exhalation was also visible, but not in so great a degree as in the case above-mentioned.

Camphor was presently reduced to a fluid state by imbibing acid air, but there seemed to be something of a whitish sediment in it. After continuing two days in this situation I admitted water to it; immediately upon which the camphor resumed its former solid state, and to appearance was the very same substance that it had been before; but the taste of it was acid, and a very small part of the air was permanent, and slightly inflammable.

As the acid air seemed to have a near affinity with phlogiston, I expected that the fumes of *liver of sulphur*, which chemists agree to be phlogistic, would have united with it, so as to form inflammable air; but I was disappointed in that expectation. This substance imbibed half of the acid air to which it was introduced;

duced; one fourth of the remainder, after standing one day in quicksilver, was imbibed by water, and what was left extinguished a candle. This experiment, however, seemed to prove that acid air and phlogiston may form a permanent kind of air that is not inflammable.

I afterwards made another experiment of the same kind, rather more decisive than the former. I put several pieces of liver of sulphur to a quantity of marine acid air; when I observed that it presently began to be absorbed, and it continued in that state till one-half of the whole had disappeared. By this time the liver of sulphur, which had been of a greenish or yellowish colour, became white. Afterwards more liver of sulphur absorbed more of this air; but after two days the pieces began to dissolve, and at length they became one liquid mass, the air still diminishing very gradually. In this state I admitted water to the air; but by this very little more of it was absorbed; and that which remained was about one-fourth of the original quantity, and extinguished a candle. The whole process was three days.

S E C -

SECTION III.

Miscellaneous Observations relating to marine acid Air.

BEING desirous of ascertaining whether the marine acid air would combine with the same substances that the marine acid dissolved, I made the trial with the flowers of zinc and red lead; and found that both these substances absorbed a very great quantity of that air. I therefore conclude that whether the marine acid be combined with water, or not, it has the same affinity with these earthy substances.

I found that two grains and a half of rain-water absorbed three ounce measures of this air, after which it was increased one third in its bulk, and weighed twice as much as before; so that this concentrated vapour seems to be twice as heavy as rain-water. Water impregnated with it makes the strongest spirit of salt that I have seen, dissolving iron with the most rapidity. Consequently, two thirds of the best spirit of salt is nothing more than mere phlegm, or water.

The following experiments are those in which the *stronger acids* were made use of, and therefore they may assist us farther to ascertain their affinities with

certain substances, with respect to this marine acid in the form of air. I put a quantity of strong concentrated *oil of vitriol* to acid air, but it was not at all affected by it in a day and a night. In order to try whether it would not have more power in a more condensed state, I compressed it with an additional atmosphere; but upon taking off this pressure, the air expanded again, and appeared to be not at all diminished. I also put a quantity of strong *spirit of nitre* to it without any sensible effect. We may conclude, therefore, that the marine acid, in this form of air, is not able to dislodge the other acids from their union with water.

Blue vitriol, which is formed by the union of the vitriolic acid with copper, turned to a dark green the moment it was put to the acid air, which it absorbed, though slowly. Two pieces, as big as small nuts, absorbed three ounce measures of the air in about half an hour. The green colour was very superficial, for it was easily wiped or washed off.

Green copperas turned to a deeper green upon being put into this acid air, which it absorbed slowly. *White copperas* absorbed this air very fast, and was dissolved in it.

Sal ammoniac, being the union of spirit of salt with volatile alkali, was no more affected with this acid air than, as I have observed before, common salt was.

When

When I put a piece of *nitre* to this air it was presently surrounded with a white fume, which soon filled the whole vessel, exactly like the fume which bursts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and such as is seen when nitrous air is mixed with this acid air. In about a minute, the whole quantity of air was absorbed, except a very little, which might be the common air that had lodged upon the surface of the spirit of salt within the phial. I have since repeated the experiment; but the result was nothing more than might have been predicted; for the nitrous acid, dislodged from its base by the marine, had dissolved some of the quicksilver, and formed nitrous air, occupying one-half of the whole space that had been filled by the marine acid air.

A piece of *alum* exposed to this air turned yellow, absorbed it as fast as the *nitre* had done, and was reduced by it to the form of a powder. It had the same effect on borax. Common salt, as might be expected, had no effect whatever on this marine acid air.

Fine white *sugar* absorbed this air slowly, was thoroughly penetrated with it, became of a deep brown colour, and acquired a smell that was peculiarly pungent.

A piece of *quick-lime* being put to about twelve or fourteen ounce measures of this acid air, and continu-

ing in that situation about two days, there remained one ounce measure of air that was not absorbed by water, and it was very strongly inflammable, as much so as a mixture of half inflammable and half common air. Very particular care was taken that no common air mixed with the acid air in this process. At another time, from about half the quantity of acid air above mentioned, with much less quick-lime, and in the space of one day, I got half an ounce measure of air that was inflammable in a slight degree only. This experiment proves that some part of the phlogiston which escapes from the fuel, in contact with which the lime is burned, adheres to it. But I am very far from thinking that the causticity of quick-lime is at all owing to this circumstance.

The remaining experiments, in which this acid air was principally concerned, are of a miscellaneous nature.

I put a piece of dry *ice* to a quantity of this acid air, taking it with a forceps, which, as well as the air itself, and the quicksilver by which it had been confined, had been exposed to the open air for an hour, in a pretty strong frost. The moment it touched the air it was dissolved as fast as it would have been by being thrown into a hot fire, and the air was presently imbibed. Putting fresh pieces of ice to that which was dissolved before, they were also dissolved immediately, and the water thus
procured

procured did not freeze again, though it was exposed a whole night, in a very intense frost.

Flies and spiders die in acid air, but not so quickly as in nitrous air. This surprized me very much; as I had imagined that nothing could be more speedily fatal to all animal life than this pure acid vapour.

Marine acid air extinguishes flame, and is much heavier than common air; but how much heavier, will not be easy to ascertain. A cylindrical glass vessel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upside down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleasing to observe the colour of the flame in this experiment; for both before the candle goes out, and also when it is first lighted again, it burns with a beautiful green, or rather light-blue flame, such as is seen when common salt is thrown into the fire.

I shall conclude my account of these experiments with observing, that the electric spark is visible in this acid air, exactly as it is in common air; and though I kept making this spark a considerable time in a quantity of it, I did not perceive that any sensible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made use of in taking the sparks.

At another time, having made about fifty electric explosions of a common jar, in a small quantity of the *marine acid air*, confined in a glass-syphon by quicksilver, I observed that it was a little diminished, and that a small part of the inside of the glass, next to the quicksilver, was tinged white. Water admitted to this air absorbed so much of it, that no experiment could be made on the remainder.

PART

P A R T II.

OF VITRIOLIC ACID AIR.

SECTION I.

The History of the Discovery.

I Had no sooner exhibited the *marine acid* in the form of air, than it occurred to me that it might be possible to exhibit the other acids also in the same elegant manner, divested of the water with which they had hitherto been combined, and which must necessarily have been a great obstruction to the discovery of their real natures and affinities; but not being a practical chemist, and living in the country, where I had no access to any person of that profession (and indeed not being sufficiently able to explain my wants) I met with many hindrances in the prosecution of my inquiries into this subject.

My first scheme was to endeavour to get the *vitriolic acid* in the form of air, thinking that it

would probably be easy to confine it by quicksilver; for as to the nitrous acid, its affinity with quicksilver is so great, that I despaired of being able to confine it to any purpose. I therefore wrote to my friend Mr. Lane, to procure me a quantity of *volatile vitriolic acid*, which is the common vitriolic acid combined with phlogiston, at the time that I was intent upon the prosecution of my former experiments; but by some mistake of my meaning, a different thing from what I intended was sent me.

Seeing Mr. Lane the winter following, he told me that if I would only heat any oily or greasy matter with oil of vitriol, I should certainly make it the very thing I wanted; viz. the *volatile, or sulphureous vitriolic acid*; and accordingly I meant to have proceeded upon this hint, but was prevented from pursuing it, by a variety of engagements.

Some time after this, I was in company with Lord Shelburne at the seat of Mons. Trudaine, at Montigny in France; where, with that generous and liberal spirit by which that nobleman is distinguished, he has a complete apparatus of philosophical instruments, with every other convenience and assistance for pursuing such philosophical inquiries as any of his numerous guests shall chuse to entertain themselves with. In this agreeable retreat

treat I met with that eminent philosopher and chemist, Mons. Montigni, Member of the Royal Academy of Sciences; and conversing with him upon this subject, he proposed our trying to convert oil of vitriol into vapour, by boiling it with a pan of charcoal in a cracked phial. This scheme not answering our purpose, he next proposed our heating it together with oil of turpentine. Accordingly we went to work upon it, and soon produced a quantity of some kind of air confined by quicksilver; but our recipient being overturned by the suddenness of the production of air, we were not able to catch any more than the first produce, which was little else than the common air which had lodged on the surface of the liquor, and which appeared to be a little phlogisticated, by its not being much affected by a mixture of nitrous air.

Having no opportunity of repeating the experiment at that time, I did nothing with a view to it till my return to England; when, on the 26th of November, 1774, I resumed the operation, beginning with *olive oil*, and by the help of a more convenient kind of glass vessel, represented fig. *a*, Pl. IV. which I had procured for these and other similar purposes, I found very little difficulty in the prosecution of the experiments.

As I wish that my reader may enjoy the benefit of my experience, I would caution him, if he chuse

to

to repeat the experiments, not to put too much oil, or any other similar substance, to the oil of vitriol, in order to produce this air. I began with using about one fifth part of common oil, leaving space enough, as I thought, in the phial, for the ebullition that might be occasioned by the production of air. But as soon as the vessel was heated to a certain degree, the production of air was exceedingly rapid; and though I withdrew the candle which I had applied to it for that purpose, the ebullition continued to increase, till, the capacity of the tube not being sufficient for the transmission of the generated air, the cork was driven out of the phial, and all the contents of it exploded.

After this I only slightly covered the spirit of vitriol in the phial with olive oil, and then the phenomena were similar to those in the former experiment, at the same time that the process was more manageable; for, by applying or withdrawing the candle, as I saw occasion, I got what quantity of air I pleased; and removing the phial, in this state of ebullition, from one vessel to another, I filled several of them with this new species of air, as easily as I had been used to do it with the marine acid air; and the whole process was as pleasing and as elegant. Indeed, this manner of producing air from substances contained in small phials, and receiving the produce in quicksilver, when it is of such

such a nature that it cannot be confined by water, has never failed to strike every person to whom I have shewed it.

The moment that I saw the acid of vitriol assume the form of air by the addition of phlogiston, I concluded that the marine acid also must have been capable of being exhibited in the same manner, by means of the phlogiston which it naturally contains, and which is inseparable from it; and moreover, that, probably, some portion of phlogiston may be necessary to the volatility and elasticity of all substances whatever; so that the marine acid air may not be precisely what I had before imagined, viz. the *pure marine acid in the form of air*; but that, though it is by this means exhibited free from water, which, in a variety of respects, modifies and restrains its action upon various bodies, it is still combined with a portion of phlogiston. Since, however, all the bodies with which we are acquainted are, in some degree, elastic, being capable, at least, of being condensed by cold, and dilated by heat, it may not be possible to separate this principle intirely from any substance in nature; and therefore, in a sense sufficiently near the truth, it may still be said that the marine acid air is nothing but the marine acid; the phlogiston it contains being so small, as not to be discoverable by any of the usual tests of its presence.

Before

Before any air is produced from the mixture of inflammable matter and oil of vitriol, the whole quantity becomes very black; and a quantity of this spirit, thus impregnated with phlogiston, will yield many times more air than an equal quantity of the strongest spirit of salt; but I never measured it with any exactness.

When the vitriolic acid air is produced in great plenty, the top of the phial in which it is generated is generally filled with white vapours. This air has also the same appearance as it is transmitted through the glass tube, and it is sometimes discoverable in the recipient.

S E C -

SECTION II.

*Of Vitriolic Acid Air from Metals, and other Substances
containing Phlogiston.*

I Got no air from the oil of vitriol by any application of heat. But in attempting to procure it, I got it by means of *mercury*, in a manner that I little expected, and I paid pretty dearly for the discovery it occasioned. Despairing to get any air from the longer application of my candles, I withdrew them; but before I could disengage the phial from the vessel of quicksilver, a little of it passed through the tube into the hot acid; when, instantly, it was all filled with dense white fumes, a prodigious quantity of air was generated, the tube through which it was transmitted was broken into many pieces (I suppose by the heat that was suddenly produced) and part of the hot acid being spilled upon my hand, burned it terribly, so that the effect of it is visible to this day. The inside of the phial was coated with a white saline substance, and the smell that issued from it was extremely suffocating.

This accident taught me what I am surprized I should not have suspected before, viz. that some
metals.

metals will part with their phlogiston to hot oil of vitriol, and thereby convert it into a permanent elastic air, producing the very same effect with oil, charcoal, or any other inflammable substance.

Not discouraged by the disagreeable accident above-mentioned, the next day I put a little *quicksilver* into the phial with the ground stopper and tube, along with the oil of vitriol; when, long before it was boiling hot, air issued plentifully from it; and being received in a vessel of quicksilver, appeared to be genuine vitriolic acid air, exactly like that which I had procured before; being readily imbibed by water, and extinguishing a candle in the same manner as the other had done. A white salt was formed; but what I thought a little remarkable, was, that, whereas in all the former experiments the oil of vitriol turned black before it yielded any air; this was not the case here; for it continued colourless and transparent during the whole process.

After this I repeated the experiment with several other metals; but with a considerable variety in the results.

Putting pieces of *iron wire* into the oil of vitriol, a very small quantity of air was produced without heat; but this soon ceasing, I applied the candle, when, with a degree of heat, seemingly greater than that at which the air had risen from the quicksilver in the same circumstances, air was produced in great

plenty. When I had got about three ounce measures of it, I admitted water to it, and about four fifths of the whole was presently absorbed. The remainder was inflammable, burning very red.

Had the oil of vitriol been more concentrated, or had I continued the process longer, a greater proportion of the air would probably have been acid, and less of it inflammable. In this experiment the oil of vitriol became very opaque, being of a deep grey colour. The iron which had undergone this process, and which I had laid aside without any expectation, was, in a few days, covered with a whitish dust; and after it had been wiped clean, was covered again with the same matter. It is very much unlike the rusting of iron in other circumstances.

About one third of the produce of air from zinc, was acid, and the remainder inflammable. Indeed it was evident that the acid had a considerable effect upon the zinc before the application of the candle, small bubbles of air continually rising from it. The oil of vitriol, which had been used in this process, after a long time, deposited a white matter, which I suppose to be the *flowers of zinc*.

Copper, treated in the same manner, yielded air very freely, with about the same degree of heat that quicksilver had required, and the air continued to be generated with very little application of more heat. The whole produce was vitriolic acid air, and no part

part of it inflammable. The oil of vitriol remained a long time turbid, but at length deposited a brownish matter.

The solution of *silver* in the same manner, had the very same result, all the air being acid, and no part of it inflammable. The oil of vitriol acquired a kind of orange-colour, and deposited nothing.

With a very great degree of heat *lead* yielded a little air, which was wholly acid, and had nothing inflammable in it.

Gold yielded no air at all in this treatment; but the oil of vitriol acquired the same orange-colour that it had when the silver had been heated in it.

Neither had this treatment of *platina* any sensible effect. What I made use of was some which I had been favoured with from Dr. Irving, carefully purged from iron.

In most of these processes, air seems to issue from the substances immediately upon the application of heat, and sometimes without it: and this first produce of air forms bubbles, which continue some time on the surface of the liquor. But it seems to be nothing more than the common air which had adhered to the surfaces of those substances, or had been confined in the little cavities near the surface, when they happened to be rough. For this seeming production of air soon ceases, and no more is produced without a much greater degree of heat;
and

and when the genuine acid air begins to rise, bubbles formed by it break instantly, like bubbles of air in spirit of wine, and there is nothing like froth on the surface of the oil of vitriol.

As sulphur is formed by the union of phlogiston with oil of vitriol highly concentrated and very hot, I imagined that by heating substances containing phlogiston in vitriolic acid air, I could not fail to produce sulphur; but I tried charcoal in this manner without the effect that I had expected from it. The heat of a burning lens thrown upon it in this acid air, only made it throw out that quantity of the air, which, as I have observed before, is condensed upon its surface, or imbibed by it. The air that was unabsorbed after this operation was in part fixed, and in part inflammable, having come from the charcoal.

When I endeavoured to procure this air by the same process from *ether*, about one half of the produce was permanent and inflammable. The oil of vitriol became perfectly black by this means, as in heating other phlogistic matters in it. Afterwards, heating the same oil of vitriol and ether, about one fourth of the produce only was inflammable; and had I continued to use the same mixture, the produce would probably have been less and less inflammable, and more purely acid, every experiment.

I once had a quantity of concentrated acid of vinegar, from which I expelled air by heat, and found it very much, if not in all respects to resemble vitriolic acid air, and being informed that oil of vitriol was used in the preparation of it, I imagine that it was vitriolic acid air only, especially as I have not found any other vegetable acid liquor that will yield air; at least in a quantity sufficient for any experiments. I tried *radical vinegar of the crystals of verdegris rectified*, which was recommended to me, and made for me, by Mr. Woulfe, and also concentrated acid made from *sal diureticus*, by Mr. Godfrey; but neither of these acid liquors, though the smell of them was extremely pungent, yielded any air by heat.

The common air expelled from the phial by the steam of this vinegar, mixed with whatever acid vapour might come over along with it, I examined, after letting it rest upon quicksilver a whole night, and I found it not to differ from common air.

When, however, I tried this experiment with air that had lodged on the surface of oil of vitriol, into which I had put some *sal diureticus*, and which did yield a little air, the common air did appear to be injured by the mixture, as in the experiment of the mixture of common and vegetable acid air. But then oil of vitriol being employed in
this

this experiment, as well as in the preparation of the concentrated vinegar above-mentioned, it is liable to the same objection; the acid of vitriol being, perhaps, volatilized by some small portion of phlogiston.

SECTION III.

Of Water impregnated with Vitriolic Acid Air.

WATER being admitted to the vitriolic acid air absorbed it about as readily as the marine acid air; and by its union with it must have formed the volatile or sulphureous acid of vitriol. Indeed the result of this combination was so obvious, that I did not think it necessary to make the experiment.

I often considered what can make the very great difference between the common vitriolic acid, and water fully impregnated with the vitriolic acid air. To judge *a priori*, from the analogy of other acids, one would imagine that there could be no great dif-

ference between them. Water impregnated with marine acid air is, in all respects, the very same thing with the common spirit of salt, except that this acid may be made considerably stronger in this manner than any spirit of salt made in the common way, and that it has generally less colour. Also water impregnated with nitrous acid vapour is the same thing with common spirit of nitre, except that the acid thus communicated is more volatile, and the colour is changed. But water impregnated with *vitriolic acid air* differs most remarkably from oil of vitriol. Its acidity is now become trifling to what it was; and from being the most fixed, and the strongest, it is now become the weakest, and the most volatile of all acids; the smell of it being intolerably pungent, and almost the whole of it evaporating when it is exposed to the open air.

This great difference seems, however, to be wholly occasioned by the phlogiston superadded to the vitriolic acid. This principle united to the acid in a manner neutralizes it, forming a kind of sulphur, in which the acidity is, in a great measure lost. To the phlogiston, also, which it has now acquired is owing its extreme volatility, to both which properties phlogiston is known in other cases to contribute. The different manner in which the vitriolic and nitrous acids combine with phlogiston,
and

and the various results from these combinations, may furnish much matter for speculation and experimental inquiry, for which we now seem to be excellently well prepared.

For a long time I concluded that water impregnated with vitriolic acid air was intirely incapable of dissolving any metal, so as to yield inflammable air; but at length I succeeded in this experiment; which convinced me that the acid is not essentially altered in this process. For having completely saturated a quantity of water with vitriolic acid air, I let it continue upon the quicksilver in which the process had been made, with a considerable quantity of superabundant acid air upon it, for about ten days; both to produce a complete saturation, and likewise to make, if possible, a more perfect combination of the acid with the water. Then, decanting it as gently as possible, and leaving it in a phial with a small perforation in the cork, that the more volatile part might escape gradually, I poured a quantity of the remainder upon some filings of zinc; upon which, with the application of heat, a considerable quantity of air was produced, and this was all strongly inflammable.

The quantity of *acid* in water impregnated with vitriolic acid air, may, in some measure, be compared with the quantity of acid in the oil of vitriol

from which it is expelled, by the method which I made use of in the following experiment. I put to a quantity of oil of vitriol rather more copper than it could dissolve; and observed that air was expelled till nothing fluid remained in the phial, and by this time the volatile acid which it had yielded had completely saturated eight or ten times its bulk of water. The residuum was blue vitriol, containing the crude oil of vitriol united to the copper.

Now if the quantity of this oil of vitriol be estimated, and this experiment be repeated with a little more care, the exact proportion of acid in the oil of vitriol, and in water fully impregnated with vitriolic acid air, may be determined. When the salt above mentioned was washed in water, a blackish matter was separated from it; but all the rest was pure vitriol, of a light green colour, but which became white, or grey, when it was dried by the fire.

The greater degree of acidity in water impregnated with marine acid air, and its greater power of dissolving metals, so as to produce inflammable air, will not be wondered at, when it is considered how much more *marine* acid air, than of *vitriolic* acid air, the same quantity of water will imbibe. In order to ascertain this, I took two small glass tubes, closed at one end, and put into each of them, as
nearly

nearly as possible, two grains of rain water, out of which the air had been pumped; nearly filling them, but so that there might be room enough for the water to expand with the vapour they might imbibe, and then introduced them into jars filled with each of the kinds of air, standing in quicksilver. I then observed that the water in the marine acid air imbibed it very fast, and diminished the bulk of it, in all, one ounce measure and three quarters, whereas the water in the vitriolic acid air imbibed it very slowly, and, in all, not more than occupied the space of three pennyweights and sixteen grains of water; so that water imbibes about ten times as much marine acid air, as of vitriolic acid air.

The water saturated with marine acid air at this time was, as far as I could perceive, quite colourless, and it was so strongly impregnated, that the warmth of my hand only made it absolutely boil by the emission of its air, and it smoked copiously.

Considering the much greater quantity of marine acid air, than of vitriolic acid air, that a quantity of water will take up, I was rather surprized to find that when water was saturated with the latter, it could not be made to imbibe a greater quantity of the former than I perceived it to do; though I was not surprized to find that when water was fully impregnated with the former, it should take but a little more of the latter, in the following experiments.

X 4

Putting

Putting vitriolic acid air to water fully saturated with marine acid air, it imbibed but little, and that very slowly ; in all, about four times its bulk. It was observable, however, that the vitriolic acid air intirely discharged the straw colour of the marine acid, and gave it a blueish tinge.

Having saturated a quantity of water with vitriolic acid air, it would not afterwards take marine acid air, except in a small quantity, and very slowly. N. B. The bubbles of marine acid air, which burst in the superabundant vitriolic acid air, which lodged on the surface of the water, emitted a white fume.

It is remarkable that water impregnated with vitriolic acid air retains all its air when it is frozen, though every other kind of air (if the liquor containing it can be frozen at all) is separated from it in the act of freezing. I have now farther observed, that this ice sinks in the liquor from which it is frozen, in which it resembles the ice of oil. This is a fact which I barely mention, without having any theory to account for it.

SECTION IV.

*Various Properties of vitriolic Acid Air.*1. *Vitriolic Acid Air and marine Acid Air compared.*

VITRIOLIC acid air is equally transparent with marine acid air, and seems to have no more affinity with quicksilver; for when confined by quicksilver, the dimensions of it are not liable to any variation, excepting by heat and cold, just like common air; provided there be no moisture in the recipient, or in the quicksilver. As the resemblance between these two acid airs was so great, it was natural for me to have a view to the experiments I had made with the marine acid air, in conducting those that relate to the vitriolic acid, which the reader will easily perceive.

Like the marine acid air, this vitriolic acid air extinguishes a candle, but without any peculiar appearance in the colour of the flame, as it goes out, or as it is lighted again, which is observable when the experiment is made with the marine acid air. Vitriolic acid air is also heavier than common air; for a candle being let down into a vessel filled with it,

it, was extinguished many times successively, and even after it had stood a full hour with its mouth exposed to the common air.

This acid air bears to be exposed to cold, without any greater diminution of its bulk than common air is subject to in the same circumstances; which appears to me to be a sufficiently proper criterion to distinguish *air* from *vapour*. In a certain degree of heat, indeed, even *water* may be exhibited in the form of air; but it is a degree of heat that far exceeds what is usual in our atmosphere; and in other cases terms are applied to very great use, for the distinction of bodies, which, if examined with strictness, would be found ultimately to run into one another, the difference between them being in *degree* rather than in *kind*: but a very *great difference* in degree affords a sufficient foundation for a difference in appellation.

2. *The Mixture of vitriolic Acid Air and Alkaline Air.*

The phenomena which attend the mixing of alkaline air with the marine acid air, were so striking, that I had not been many hours in possession of the vitriolic acid air without trying whether the effect of the same mixture with this acid air would not make a similar appearance, and the experiment

fully answered my expectations. A like beautiful white cloud was formed the moment that these two kinds of air came into contact, the quantity of air was diminished as fast as the alkaline air was admitted, and the quicksilver rose almost to the top of the receiver.

I observed also, that when I put the alkaline air to the vitriolic acid air, the white cloud rose immediately to the top of the vessel, as in the experiment with the marine acid air; which proves that the alkaline air is, in both cases, the lighter of the two. In both cases also, if the alkaline air be produced first, the acid air being admitted to it, forms a cloud which rests upon the quicksilver; never extending beyond a very small space, and rising only as the quicksilver rises. The substance that is formed by the union of the alkaline air with the vitriolic acid air, must necessarily be the *vitriolic sal ammoniac*; but I made no experiment to ascertain it. The quantity of this salt with which my receivers are coated in these experiments is readily dissolved in water, as in the experiments with the marine acid air. This, however, it will be seen, is not the case with the salt that is formed by another of the acid airs with alkaline air.

There was frequently, however, the appearance of sulphur produced upon the mixture of alkaline air with vitriolic acid air; for the inside of the tube
would

would be covered with a perfectly yellow matter. But this colour goes off in time, and nothing but a white saline substance remains. This yellow appearance I first observed when I had produced the vitriolic acid air from ether; but afterwards I found the same effect when it was produced from charcoal, and still more remarkably when it had been produced from copper. Why this yellow colour should not be permanent, I do not understand.

Being willing to try whether, by making repeated mixtures of these two kinds of air, I could not fix this colour, and collect a quantity of the substance in which it inheres, I filled the same jar alternately with each of these kinds of air, till all the inside of the vessel seemed to have got a good coating of this substance. But, being exposed to the open air, the colour in this case also soon disappeared, and the saline substance with which all the inside of the tube was uniformly coated, became perfectly white.

3. *Vitriolic Acid Air mixed with other Kinds of Air.*

The mixture of other kinds of air with vitriolic acid air produced no remarkable appearance whatever. When, however, I had put a quantity of this acid air to a quantity of common air, in order to observe whether the former might not part with
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some of its phlogiston to the latter, though I perceived no immediate diminution of the bulk of air, as in the mixture of nitrous and common air; yet when they had continued together two days, and water being admitted to the mixture had absorbed the acid air, the common air which remained appeared, by the test of nitrous air, to be considerably injured; so that the phlogiston in the vitriolic acid air must have united to some part of the pure air contained in the common air, which is an effect that is not produced by the marine acid air when mixed with common air. In reality, the vitriolic acid air must have attracted dephlogisticated air, from the atmosphere, and have thereby formed the common vitriolic acid. What effect the vitriolic acid air would have had upon other kinds of air, had they continued together a longer time, I cannot tell.

A quantity of this acid air mixed with inflammable air stood some hours; but when water had been admitted to them, I could not perceive either that the quantity of inflammable air was altered, or that its inflammability was in the least impaired.

I once put equal quantities of marine and vitriolic acid air into the same receiver, and observed that they mixed without exhibiting any appearance whatever; and when alkaline air was admitted to them, the appearance was the same as if it had been admitted to either of them singly, the white cloud

cloud rising instantly to the top of the vessel. Had I, after the experiment, examined the *salts* which adhered to different parts of the inside of the vessel, I might perhaps have discovered which of the two acid airs was specifically lighter than the other; but I suspect that they were intimately mixed, and therefore that the salt was a mixture of the common and the vitriolic sal ammoniac.

4. *Substances containing Phlogiston exposed to vitriolic Acid Air.*

I thought it rather extraordinary, that whereas the marine acid, which is reckoned the weakest of all the three mineral acids, should, when exhibited in the form of air, be able to dislodge both the vitriolic and the nitrous acids from several of their bases; yet that this vitriolic acid, which is reckoned the strongest of the three, when seemingly exhibited to equal advantage, by being divested of the water with which it is usually combined, should not, in any instance in which I made the experiment, dislodge either of the other acids from any basis with which they were united. *Nitre, common salt, and sal ammoniac*, were all introduced to this air, without either affecting it, or being affected by it.

Oils imbibe vitriolic acid air, and pretty rapidly, in proportion to their tenuity; though they cannot be made to imbibe so much of it as *water*
can

can. Being willing to see the utmost effect of the impregnation of several kinds of oil with this acid air, I took about equal quantities of oil of turpentine, olive oil, and whale oil, and gave each of them an opportunity of being fully saturated; having kept them constantly supplied with fresh air, when they had imbibed a former quantity, and frequently discharging that part of the air which they could not imbibe, and beginning the process a new.

Both the whale oil and the olive-oil imbibed this air very slowly, being several hours in taking their bulk of it; but whereas the first effect of the impregnation of the whale-oil was imparting to it a reddish colour, the olive-oil became quite colourless. In all, they both imbibed six or eight times their bulk of this air.

In this manner they continued in glass jars confined by quicksilver about a month, when I observed that the oil of turpentine was become of a beautiful amber colour, the olive-oil was darker, and the whale-oil darker still; and they all smelled extremely pungent, by the escape of the acid air from them. Their tenacity was not seemingly increased.

Vitriolic ether imbibes vitriolic acid air as readily as water imbibes it. The ether, however, was soon saturated with it, and afterwards was, to all appearance, both as transparent, and as inflammable as before.

A piece

A piece of *phosphorus* remained a day and two nights in vitriolic acid air, without sensibly affecting it. It gave no light in this air; but the upper surface of it turned black, and the surface of the quicksilver on which it lay, had a deep yellow or blackish kind of scum upon it, as if it had been in part dissolved by the acid.

A piece of *liver of sulphur*, in three days, absorbed the whole of a quantity of this kind of air, without sensibly affecting the colour or appearance of the liver of sulphur.

Charcoal, which forms inflammable air, by being introduced to marine acid air, only absorbs the vitriolic acid air; which, however, it does pretty rapidly, and acquires a pungent smell from being exposed to it, without producing any other effect that I could perceive. I made several pieces of charcoal imbibe as much of this acid air as they could; but, after this, fresh pieces absorbed the remainder, so that the air had only been, as it were, condensed on its surface. This I have observed to be the case with alkaline air, and in some experiments with other kinds of air which cannot be confined but by quicksilver; and I do not clearly understand it. The charcoal, in this experiment, was made very dry, or it might have been suspected that the moisture adhering to it had absorbed the air.

Vitriolic

Vitriolic acid air dissolved *camphor* pretty readily, and reduced it to a transparent liquor. Water being admitted to it, the camphor re-assumed its natural solid form, but seemed to have acquired an acidity in its taste.

Iron is readily dissolved in marine acid air, but it is not at all affected by the vitriolic acid air; though, when combined with water, it is so powerful a menstruum for iron. But this, indeed, is the less extraordinary, as this acid ceases to affect iron when it is strongly concentrated. I kept a number of iron nails in vitriolic acid air two days, without any sensible effect either upon the air, or the nails. There was no appearance of their being in the least corroded.

I have noticed a very remarkable effect of alkaline air upon alum, rendering it white and opaque, as if it had been calcined, but without altering its figure. The same, to appearance, is the effect of vitriolic acid air upon *borax*. This substance absorbed a pretty large quantity of this air in two days. What remained of the air extinguished a candle. But this effect was probably owing to a small proportion of fixed air that was produced at the same time with the vitriolic acid air. I repeated this experiment with borax, and let the process continue three days, when the effect was precisely the same as before, the borax retaining its form, but be-

ing rendered white and opake. The acid air had, no doubt, seized upon the water which enters into its composition, as I conjecture to be the case with respect to alkaline air and alum.

As it is well known that the common vitriolic acid is changed into volatile or sulphureous acid of vitriol by fumes of charcoal, if the vessel in which it is heated has a crack in it, through which the fumes can have access to the acid, I had the curiosity to try whether the same effect would not be produced by heating the charcoal in the acid. Accordingly I put some bits of *charcoal* into my phial, instead of the oil, or other inflammable matter, which I had used before; and, applying the flame of a candle, I presently found that the vitriolic acid air was produced as well as in the former process, and in several respects more conveniently, the production of air being more equable; whereby the disagreeable effect of a sudden explosion is avoided.

It is necessary, however, that the charcoal be very well burned, so that all air be expelled from it; otherwise, there will be a mixture of fixed or inflammable air along with the acid air, especially when a considerable degree of heat is applied to produce the air. Having often got vitriolic acid air from charcoal on account of the easy and equable production of it in this manner, I several times observed that there was a considerable residuum after

it

it had been exposed to water: and once I found that the residuum made lime-water turbid; a sure sign of its containing fixed air.

SECTION V.

Of taking the Electric Spark in Vitriolic Acid Air.

ONE of the most remarkable observations that I have made on vitriolic acid air was, that when the electric spark is taken in it, the inside of the glass tube in which it is confined is covered with a blackish substance; so that the inside of the glass through which the explosion passed was uniformly covered with a blackish matter, and nothing could be seen through it, and the air seemed to be rather increased than diminished. Water being admitted to it, left so little of it unabsorbed, that it could not be examined. Part of the blackish matter was washed off by the water.

I have since given more attention to this curious circumstance, and have been happy enough to ascer-

tain several things of considerable importance relating to it, though they are not sufficient for a complete theory of it.

My first wish was to collect a quantity of this matter, enough for a chemical examination of it; and for this purpose I applied to my friend Mr. Vaughan, who favoured me with his assistance, and the use of his large machine, made by Mr. Nairne, on the same plan with what which was made for the Grand Duke of Tuscany, described in the Philosophical Transactions. But though we gave a good deal of time to this business, we did not get a quantity sufficient for my purpose.

I saw reason, however, to conclude, that the whole of the vitriolic acid air is convertible into this black matter, but not by means of any union which it forms with the electric fluid; but as it should seem, in consequence of the *concussion* given to it by the explosion; and that, if it be the calx of the metal which supplied the phlogiston, it is not to be distinguished from what metal, or indeed from what substance, of any other kind, the air had been extracted. These particulars will be sufficiently evinced in the following experiments.

I made 120 explosions of a common jar in about a quarter of an ounce measure of vitriolic acid air
from

from copper ; by which I found that the bulk of it was diminished about one third, and the remainder was seemingly not changed, being all absorbed by water. In the course of this process the air was carefully transferred three times from one vessel to another ; and the last vessel in which the explosions were made in it was, to all appearance, as black as the first ; so that this air seems to be all convertible into this black matter, whatever it be.

Thinking this diminution of the vitriolic acid air might possibly arise from its absorption by the cement, with which the glass tubes employed in the last experiment were closed, I repeated it, with the variation only of getting the air from quicksilver, in a *glass syphon*, confined by quicksilver ; and observed that it was diminished, by the same number of explosions, in about the same proportion as before.

That this matter comes from the vitriolic acid air and not from any combination of the electric matter with it, will, I think, appear from the following experiment. I took the simple electric spark from a prime conductor of a moderate size, for the space of five minutes, without interruption, in a quantity of vitriolic acid air, without producing any change in the inside of the glass ; when, immediately after, making in it only two explosions of a common jar, each of which might be produced in less than a quarter of a minute, with the same machine,

chine, in the same state, the whole of the inside of the tube was completely covered with a black matter. Now, had the electric matter formed any union with the air, and this black matter had been the result of that combination, all the difference that could have arisen from the simple *spark*, or the *explosion*, could only have been a more gradual, or a more *sudden* formation of that matter.

Had I used tubes small enough, or a prime conductor large enough, I should, no doubt, have produced this blackness with the sparks only. On the other hand, when I had filled a large phial, about an inch and a half wide, with this air, I found that even the explosion of a very large jar, containing more than two feet of coated surface, had no effect upon it; from which it should seem that, in these cases, the force of the shock was not able to give the whole quantity of air such a concussion as was necessary to decompose any part of it.

At one time I had little doubt, but that the matter formed by these explosions in vitriolic acid air would be different, according to the calx of the metal, or other substance which had supplied the phlogiston of it; imagining that, together with phlogiston, some of the earthy part of the substance had also entered into the composition of the air, and re-appeared in this decomposition of it, in the form of a powder. But, upon the whole, I did not find
that

that this conjecture was verified by the facts, which I shall here recite.

I had generally made use of *copper*; but when I got a quantity of this air from *quicksilver*, in the experiment mentioned above, and also when afterwards, I, for this very purpose, procured this air from almost all the kinds of substance from which it can be procured, the electric explosion taken in it produced the same effect, in clouding the tube with the black matter, as it had done in the air procured from copper. As some of these experiments were attended with peculiar circumstances, I shall briefly mention them.

I was most embarrassed when I endeavoured to get vitriolic acid air from *lead*, putting a quantity of leaden shot into a phial containing oil of vitriol, and applying only the usual degree of heat, a considerable quantity of air was produced; but afterwards, though the heat was increased till the acid boiled, no more air could be got. I imagine, therefore, that, in this case, the phlogiston had, in fact, been supplied by something that had adhered to the shot. However, in the air that was so produced, I took the electric explosion, and in the first quantity that I tried, a whitish matter was produced, almost covering the inside of the tube. But in the succeeding experiments with air procured from the same shot, or from something adhering to it, there

was less of the whitish matter; and at last nothing but black matter was produced, as in all the other experiments. Water being admitted to this air, there remained a considerable residuum, which was very slightly inflammable.

Vitriolic acid air is easily procured from *spirit of wine*, the mixture becoming black before any air is yielded. The electric explosion taken in this air also produced the black matter.

If any of my experiments be capable of throwing light upon the subject of this black matter, it should seem to be those which I made with *ether*, by the help of which this air is as easily procured as by any other substance containing phlogiston. In the air procured by ether the electric explosion tinged the glass very black; I think more so than I had observed in any other experiment of the kind; and when water had absorbed what it could of this air, there was a residuum in which a candle burned with a lam-bent blue flame. But what was most remarkable in this experiment was, that besides the oil of vitriol becoming very black during the process, a black substance, and of a thick consistence, was formed, which swam on the surface of the acid. I collected a quantity of it, but I have not examined it with sufficient attention. It may easily be procured by boiling ether in the vitriolic acid.

The electric spark, or explosion, taken in *common air*, confined by quicksilver, in a glass tube, covers the inside of the tube with a black matter, which, when heated, appears to be pure quicksilver. This, therefore, may be the case with the black matter into which I supposed the *vitriolic acid air* to be converted by the same process, though the effect was much more remarkable in that than in common air. The *explosion* will often produce the diminution of the common air, in half the time that the *simple sparks* will do it, the machine giving the same quantity of fire in the same time. Also the blackness of the tube is much sooner produced by the shocks than by the sparks. When the tube considerably exceeds three tenths of an inch in diameter, it will sometimes become very black without there being any sensible diminution of the quantity of air.

I have since found that this black matter is *mercury superphlogisticated*, the vapour of this metal, mixing with the air in all temperatures. This will be evident from the experiments under the article *mercury*.

SECTION VI.

Of the Convertibility of vitriolic Acid Air into vitriolic Acid.

THE volatile vitriolic acid, though produced from the fixed vitriolic acid, is very considerably different from it, especially as it may be dislodged from its basis by the vitriolic acid, just as other weaker acids are dislodged by those that are thence called the stronger. But that volatile vitriolic acid is capable, however, of being brought back to the state of the common vitriolic acid, and becoming the same thing that it originally was, several experiments shew. I had found that it was capable of dissolving iron and zinc, and of producing inflammable air, which is the property of oil of vitriol; but I had a more decisive proof of the same thing when, to water saturated with vitriolic acid air, I had, for another purpose, put some earth of alum till it was saturated. For, after six months, in which this solution had been exposed in an open phial, and one third of it was evaporated, I observed many transparent crystals formed

ed at the bottom of the phial, as well as an incrustation on the sides of the phial above the surface of the liquor. These crystals were all triangular, of a considerable thickness, connected with each other, and when examined appeared to be *alum*, which is known to be the saline substance formed by the same earth, and the proper vitriolic acid. But the following experiments, in which it will appear, that real sulphur is formed by means of the volatile vitriolic acid, exhibit a much more remarkable fact, and is another proof of the mutual convertibility of these acids into one another.

Having exposed various liquid substances to a continued heat in a sand furnace, among others I placed in it a glass tube, about an inch in diameter at the bottom, tapering to a point at the top, about two feet and a half long, closed hermetically; when I had put into it about an ounce measure of distilled water strongly impregnated with vitriolic acid air, with nothing more than a random expectation of some change or other taking place in it. This was on the 9th of September 1777, but the result was much more curious than I could possibly have imagined *a priori*. I shall note the appearances as I observed them, at the several intervals in which I examined this tube.

On the 30th of the same month this impregnated water, which continued transparent to the
end

end of the process, had deposited a small quantity of black powder; and also a bit of matter exactly like *sulphur* about one eighth of an inch in diameter lay among it. Small pieces of the same matter floated on the surface of the liquor, and streaks of the same coated part of the inside of the tube an inch above the liquor. From the top of the tube to within about eight inches above the liquor, were beautiful white crystallizations, like *spicula*, disposed irregularly, but generally in the form of stars, the glass being perfectly transparent between them.

In this state the tube continued, the crystallizations increasing, and several times changing their places, to the 20th of January following, when an end was put to the process. Excepting, however, a place of a few square inches near the surface of the liquor, all the lower half of the tube was quite free from them, but from thence to the top it was pretty thick and equally covered, exhibiting a most pleasing appearance.

In order to observe the *time*, and the *manner* of the formation of these crystals, in a greater variety of circumstances, I placed in the sand furnace at different times, a strong glass tube about nine inches long, and a quarter of an inch in diameter, which I sunk pretty deep in the sand, in order to give it a greater degree of heat; and also two tubes
about

about four feet long, one of them half an inch, and the other a quarter of an inch in diameter, putting into the short tube a quantity of the impregnated water about an inch in length, and into the long tubes two inches and a half.

The short tube had been put into the sand on the 11th of August, and on the 30th of September following the liquor was transparent, but the top and part of the middle of the tube had many white stars like crystallizations.

Of the long tubes the smaller had begun to have crystallizations, about one third from the bottom in about a fortnight, and the wider in about a month. When they were examined on the 19th of January, 1778, the large tube had more crystallizations than the smaller, the greatest quantity of them about five inches above the surface of the liquor, but they were all on one side of the tube, and there were others about six inches above these. There were also very many between the surface of the liquor and two inches above it. The smaller tube had no crystals near the surface of the liquor, but a good many about five inches above it, and the greatest quantity was about eighteen inches above it. Neither of these tubes had any crystals in two thirds of the upper part of them.

Applying the flame of a candle with a blow-pipe to the smaller of the long tubes above-mentioned,

tioned, the glass was pressed violently inwards; so that it was evident there was a decrease of elastic matter within the tube, which therefore probably entered into the crystals. If any part of the liquid touched the hot glass, a dense white fume was excited, exactly like that from the oil of vitriol. Taking off one half of the tube, and then opening it under water, it was half filled with water, and the air within in it was completely phlogisticated, which agrees with my former observations, of the vitriolic acid air imparting phlogiston to common air, or rather of its imbibing dephlogisticated air, and thereby forming vitriolic acid.

When I heated the dry crystals, the same white cloud was raised, and the crystals were by this means dispersed into a kind of dust, that incrusts the glass. For I applied the heat on the outside of the tube.

The liquor itself was still extremely acid, and the smell of it very pungent; so that, probably, only a small part of the vitriolic acid air with which it was impregnated had entered into these crystals, numerous as they were.

The crystals were easily shaken off from the side of the tube, when it was washed with the liquor, and they continued undissolved in it.

The preceding observations were made presently after the tubes in which the crystals were formed
were

were taken from the sand furnace; and in this state they continued near a year, in the course of which I had shewed them to several of my chemical friends, who expressed much surprize at the sight of them. At length I opened the tube that contained the greatest quantity of these crystals, first observing that, when I softened the glass, it was still pressed inwards.

The crystals, I found, were not dissolved in spirit of salt, and when they had been washed, and dried, they had the colour, and smell of *sulphur*; and being laid on a hot iron burned with a blue flame, so as to leave no doubt of the identity of the substances.

To form this sulphur, I conjecture, that the volatile acid, in this expanded and confined state, uniting with the dephlogisticated air in the tube, had first formed vitriolic acid; and that this, uniting with phlogiston, formed the sulphur. The fact is certainly a remarkable one.

Having observed these curious effects of the impregnation of water with vitriolic acid air, I exposed to the same heat, in similar circumstances, spirit of wine, and oil of turpentine, saturated with the same kind of air.

The impregnated spirit of wine, after being exposed to this heat about a fortnight was transparent, but had many slender crystals in it, and the greater part of the tube had a thick and whitish incrusta-

tion,

tion, beginning about three inches above the surface of the liquor, and extending about twelve inches, but was thickest in the middle.

A short tube, containing a quantity of the same impregnated spirit of wine, had no incrustation, but many more crystals, in the form of *spicula* which settled to the bottom of the liquor. Another tube of the same length had similar *spicula*, and near the top a considerable incrustation not spiculine.

The oil of turpentine impregnated with vitriolic acid air, and exposed to the heat in this manner, from being of a light amber colour, became of a deep brown. The tube in which it was contained was only eighteen inches long, and the upper half of it was covered on one side with white incrustations not spiculine.

Whale oil impregnated with this air, from being brown, had probably become almost black. For the tube was broke, but had a very black incrustation towards the bottom, especially near the surface of the liquor.

I also exposed to the same heat tubes containing vitriolic acid air only, having first filled them with quicksilver, then with this kind of air, and afterwards sealing them hermetically with a blow-pipe; and the result was similar to those in which the impregnations were concerned.

One tube of this kind that had been buried in the hot sand on the 11th of August, being examined on the 30th of September, was found in the following state. The upper part of the tube was half covered with white crystals, but much smaller than those in the tubes containing the water impregnated with this air.

Another tube containing the same kind of air, which had been buried in the sand a longer time, was found quite covered with white crystals, and a small part of the tube was black, probably from some external accidental cause. The end of this tube being broke under quicksilver, it filled one third of it, and water absorbed all that remained of the air, except a very small bubble. This water had the smell of water impregnated with vitriolic acid air.

I have several times repeated this experiment, and have never failed to find the inside of the tubes that had been filled with vitriolic acid air coated with this white matter; but it is so exceedingly slight, that I cannot make many observations upon it. I am rather surprized to find that it does not seem to be *sulphur*, which is formed from the heating of water impregnated with the same kind of air. For spirit of salt seems to dissolve it all. At least the tube is washed perfectly clean with it,

and I could not discern any thing in that acid afterwards. But this may be owing to the very small quantity of it, though it be spread on so great a surface, and to the extreme minuteness of the particles of which it consists.

Another tube containing the same kind of air, which had been buried in the sand a longer time, was found quite covered with white crystals, and a small part of the tube was black, probably from some external accidental cause. The end of this tube being broke under quicksilver, it filled one third of it, and water absorbed all that remained of the air, except a very small bubble. This water had the smell of water impregnated with vitrollic acid air.

I have several times repeated this experiment, and have never failed to find the inside of the tubes that had been filled with vitrollic acid air covered with this white matter; but it is so exceedingly fine, that I cannot make many observations upon it. I am rather surprized to find that it does not seem to be sulphur, which is formed from the burning of water impregnated with the same kind of air. For spirit of salt seems to dissolve it all. At least the tube is washed perfectly clean with it.

PART III.

OF FLUOR ACID AIR.

SECTION I.

The Discovery of the Fluor Acid Air, and the Impregnation of Water with it.

THE philosophical part of the world have, of late, been highly gratified by the discovery of a new mineral acid, contained in a substance which chemists distinguish by the name of *fluor*; but many of my readers will understand me better, when I inform them, that it is of that species of substance, which, with us, is called *the Derbyshire spar*; and of which, at present, vases and other ornaments for chimnies, are usually made. The acid is expelled from this substance by oil of vitriol, and has peculiar properties, as remarkable as any of the three other mineral acids which we were acquainted with before.

This curious discovery was made by Mr. Scheele, a Swede; from which circumstance the acid is often distinguished by the name of the *Swedish acid*. His method of operating upon this substance, and likewise that of all who have succeeded him in the inquiry, was to distil it in glass vessels, as in the process of making spirit of nitre from salt-petre; and the most remarkable facts that have been observed concerning it are, that the vessels in which the distillation is made are apt to be corroded; so that holes will be made quite through them; and that when there is water in the recipient, the surface of it will be covered with a crust, of a friable stony matter.

This crust, which I shall distinguish by the name of the *fluor crust*, Mr. Scheele supposed to be *quartz*; and therefore concluded that this acid and water were the constituent parts of that fossil. On the other hand, Mr. Boulanger, who has taken a great deal of pains with this subject, is of opinion that this new acid is only the *acid of salt*, combined with an earthy substance. For this opinion he advances various reasons; but does not pretend to be able to produce any decisive proof. The result of my own experiments, I think, prove, that the fluor contains a peculiar acid.

As soon as I had exhibited one of the acids in the form of *air*, I had no doubt but that all the acids

acids might be exhibited in the same manner, and this among the rest; but I imagined that I should find great difficulty in procuring the fossil that contains it; supposing that it had only been found in Sweden; and I should probably have continued in this incapacity for making the following experiments, had I not been relieved by Mr. Woulfe, who, upon my inquiry concerning it, not only explained to me what the substance was, but immediately furnished me with a quantity of several kinds of it, sufficient for my purpose. That with which my first experiments were made, was that which he called *the white phosphoric spar, from Saxony*; but afterwards I made use of the Derbyshire spar; and the pieces that I had by me were partly white, or yellowish, and partly purple.

All my advantage in the investigation of this subject, has arisen from my peculiar manner of conducting the experiments. For, by exhibiting the acid in the form of air, free from moisture, I had an opportunity of examining its nature and affinities with the greatest ease and certainty. In this manner also, this species of air exhibits a variety of striking phenomena, which cannot be produced in any other manner of operating upon it.

When I began these experiments, I followed the directions given by those who had gone before me in the investigation of this subject, and who

had procured the acid in the common method of distillation, pounding the fluor (which I afterwards found not to be necessary) and pouring oil of vitriol upon it. This I did in a phial, to which was fitted a ground-stopper and tube, and immediately found, that, at first, without any heat, and afterwards with a very small degree of it, air was produced in great plenty, perfectly transparent, and confined by quick-silver, like the other acid airs. The vapour, as it issued out of the tube into the open air, formed a permanent white cloud; no doubt, by attaching to itself the water that floated in the atmosphere, and the smell of it was extremely pungent.

I had no sooner produced this new kind of air, but I was eager to see the effect it would have on *water*, and to produce the stony crust formed by their union, as described by Mr. Scheele; and I was not disappointed in my expectations. The moment the water came into contact with this air, the surface of it became white and opake, by a *stony film*, which, forming a separation between the air above, and the water below it, considerably retarded the ascent of the water, till the air, insinuating itself through the pores and cracks of this crust, the water necessarily rose as the air diminished, and breaking the crust, presented a new surface of water, which, like the former, was instantly covered with a fresh crust. Thus was one stony incrustation

tion formed after another, till every particle of the air was united to the water, and the different films being collected, and dried, formed a white powdery substance, generally a little acid to the taste; but when washed in much pure water, was perfectly insipid.

Few philosophical experiments exhibit a more pleasing appearance than this, which can only be made, by first producing the air confined by quicksilver, and then admitting a large body of water to it. Most persons to whom I have shewn the experiment have been exceedingly struck with it. It is exhibited to the most advantage, when the vessel that contains the air is pretty wide, by which I mean about an inch in diameter. In this case the crust will often crack in the middle, and a small jet of water rushing through the fissure, will, to appearance, be instantly converted into this stony substance, and look like a puff of white powder, rising sometimes an inch or two up into the air. Also the *crystallizations*, formed on the sides of the vessel, as the water rises in it, make a very beautiful appearance.

The union of this acid air and water may also be exhibited in another manner, which, to some persons, makes a still more striking experiment; viz. by admitting the air, as fast as it is generated,

to a large body of water resting on quicksilver, instead of introducing the water to the air previously formed.

For this purpose, I usually put two or three ounce measures of water into a tall cylindrical jar, about an inch in diameter (such as those which I generally use as recipients of those kinds of air that must be confined by quicksilver) and filling the remainder of the vessel with quicksilver, I place it inverted in a basin containing a quantity of the same fluid; so that the water, immediately rising to the top, occupies the upper part of the vessel, while the quicksilver occupies the lower part. I then introduce under it the end of the tube proceeding from the phial, which contains the materials for generating this air. It is, then, very pleasing to observe, that the moment any bubble of air, after passing through the quicksilver, reaches the water, it is instantly, as it were, converted into a stone; but continuing hollow for a short space of time, generally rises to the top of the water, in the form of a bubble, or a thin white film. If the succession of bubbles be rapid, and they rise freely to the top of the vessel, through a large body of clear water (which, however, is not always the case, as they will sometimes adhere to the upper surface of the quicksilver) I have met with few persons

persons who are soon weary of looking at it; and some could sit by it almost a whole hour, and be agreeably amused all the time.

Every bubble of air, coming into contact with the water on every side at once, is like a bladder, being hollow within; but this slight crust soon bursting, the sides collapse, and it rises to the top of the vessel, in the form of a piece of thin white gauze. The water soon penetrating every part of it, the whole mass of these films becomes in a little time like a jelly, which continually thickens by the accession of more films, till at length the whole body of water seems to become solid; so that, being fully saturated, especially at the lower part, the air, finding no more moisture within its reach, will fill all the lower part of the vessel, expelling the quicksilver, while the water, in the form of a stiff jelly, occupies all the upper part of the vessel.

As, for the purposes to be mentioned here-after, I have repeated this experiment a great number of times, I have had an opportunity of observing a very great variety in the appearances which it exhibits. One is peculiarly pleasing, but not very common. A large bubble of air will sometimes adhere, by its lower part, to the surface of the quicksilver; and another bubble, rising in the same place, before the lower part of the former has been closed, pushes out the upper part of it, and advancing

vancing farther into the water, extends the bubble in length: another follows, and does the same, till at length a *tube* is formed (the sides also growing thicker continually) extending from the quicksilver, to the top of the water. I have seen of them four inches in length; and others being formed close to them, the whole vessel has been almost filled with these tubes, adhering to one another, of different lengths, and not much unlike the appearance of the pipes that are placed in the front of an organ.

In less than an hour, I have frequently converted two or three ounce measures of water into this solid mass. When this is taken out of the vessel; and pressed, it will be found to contain a great deal of an acid liquor; the water impregnated with the acid having been intrangled in the interstices of the jelly, out of the reach of the air: and if this liquor be used in another process, instead of pure water, more of it will seem to become solid, and the acid liquor will be concentrated every time.

By the repetition of this process, an acid liquor may be procured of a very considerable degree of strength. There seems, however, to be a limit to its strength; for the acid is exceedingly volatile, as is evident from its extremely pungent smell; so that I have thought that I gained nothing by repeating the process more than eight or ten times; because it was impossible to transfer the water from

one

one vessel to another, but more acid would be lost by evaporation, than would be acquired by another impregnation with the acid air.

These appearances I now explain, by supposing that the acid of the spar dislodged by the vitriolic acid, in uniting with the spar, is in part volatilized, appears in the form of air; and that there is also combined with this air, a portion of the solid earthy part of the spar, which continues in a state of solution, till, coming into contact with the water, the fluid unites with the acid, and the earth is precipitated.

Before I proceed to relate any of the experiments which I made with this acid air, I shall give a few directions and precautions, which may be useful to persons on their first entrance upon this course.

1. The tube through which this acid vapour is conveyed should not be very narrow, because it is apt to be furred up, especially when any phial, containing materials for the production of this air, has been used some time, and with a good deal of heat; owing, I suppose, to the hot air retaining in solution more of the stony matter than it can do when it is cold, and therefore depositing it as it is conveyed through the tube.

2. I began these experiments with phials which had ground stoppers and tubes, but soon found that it was too expensive a mode of experimenting with

this kind of air ; for they were presently corroded and spoiled. Afterwards, therefore, I used only common phials, but the thickest that I could meet with ; and still seldom found that they would bear the experiment above an hour. Very frequently, the thickest phials that I could get would be worn quite through in a quarter of an hour, when the heat was considerable, and the production of the air rapid. This power of dissolving glass is a very remarkable property of this air ; but it seems to possess it only when it is hot, at least in any considerable degree.

3. When I wished to produce this air pretty fast, I found it most convenient to pound the spar, and pour the oil of vitriol upon it, filling one fourth of the phial with the spar, and leaving one fourth of it for a space in which the bubbles might expand themselves, and break, so as not to carry any of the liquor into the tube. I now proceed to the particular experiments.

SEC.

SECTION II.

Experiments made with a View to discover the Constitution of Fluor Acid Air.

I Thought it might possibly contribute to decide the question concerning the identity of this acid and the marine, if I put a quantity of the *fluor crust* to marine acid air; thinking that they might form an union, and constitute this fluor acid air. And, indeed, something similar to it was by this means produced; so that another crust was formed upon the admission of water to it; but, in other respects, several circumstances, which I cannot explain, attended the experiments. They were as follows.

To about two ounce measures of marine acid air I put about a quarter of a grain of the fluor crust, and in about three days it had absorbed about half an ounce measure of the air. Water being then admitted to it, left a quarter of an ounce measure of air unabsorbed.

Afterwards I conveyed marine acid air to a pretty large quantity of the fluor crust, confined by quick-

silver; and, as the air was imbibed, I continued to throw up more, till, after three or four days, that substance seemed to be fully saturated with the air. Then admitting water to it, it was absorbed exactly like the fluor acid air; but I could not, at that time, very well distinguish the crust on the top of it, on account of the jar being almost filled with the crust, and part of it floating on the top of the water. About three fourths of this air was absorbed by the water; but what I thought very remarkable, air kept issuing from this fluor crust, in large bubbles, till the quantity of air was doubled, and the jar was half filled with it. This air neither affected common air, nor was affected by nitrous air, and it extinguished a candle.

I repeated the experiment, with this only difference, that I admitted water to the air as soon as the fluor crust seemed to be saturated; when the experiment being made in a wide jar, the crust on the surface of the water was as visible as in the experiment with the fluor acid air itself. At this time, however, there was no generation of air from the saturated crust, as before, but a considerable quantity of air, unabsorbed by water, though I took care that the marine acid air was as pure as I could procure it.

Having a quantity of the fluor crust saturated with marine acid air, I had the curiosity to pour
some

some oil of vitriol upon it, in order to try whether the produce would be pure marine acid air, or a mixture of the two; and the latter seemed to be the case, though I think the marine acid prevailed in the mixture.

In this process air was produced in great plenty, and the bubbles burst in the receiver with a white cloud; but when water was admitted to it, it was absorbed without any crust being formed upon its surface.

In twenty four hours a piece of salt-petre turned yellow in this air, and absorbed about half an ounce measure of it. What remained unabsorbed by water, was exceedingly strong nitrous air, the spirit of nitre having been set loose from the salt-petre by the marine acid air, and having dissolved the quicksilver.

A piece of *borax*, in about a fortnight, absorbed about two ounce measures of this air, without leaving any residuum not absorbed by water. The surface of the borax was become soft, but by washing it in water, the soft part was easily separated from the rest.

At the beginning of my investigation of this subject, I had a suspicion that this new acid air might possibly be the *vitriolic acid air*, loaded with the sparry crust; but the following experiments were not favourable to this hypothesis.

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I threw the focus of a burning lens upon some pieces of the spar in vitriolic acid air, confined by quicksilver; thinking, that when it was hot, it might dissolve some part of it, and thereby become the same thing with the fluor acid air. But though I continued this operation till the spar smoked, and filled the vessel with a white fume, there was neither any addition made to the quantity of air, nor any change produced in the quality of it. When water was admitted to it, no crust, as I had expected, was formed on the surface of it.

In order to try whether the fluor crust was the same thing with the spar, from which it had been produced, I got a quantity of it, and treated it in the same manner as I had treated the spar, pouring oil of vitriol upon it, and endeavouring to expel air from it. I presently found, indeed, that it yielded great plenty of air; but not finding it to be the thing I was then in quest of, viz. an acid air, by means of which a crust would be formed on the surface of the water admitted to it, I neglected to give sufficient attention to it.

Water admitted to this acid air, procured from the fluor crust by oil of vitriol, absorbed it all, but without having any crust upon its surface.

Alkaline air united with the whole of this acid air, forming with it a white saline substance; and part of the inside of the tube in which the mixture

was

was made, was tinged with a deep yellow, or orange colour, which disappeared after a few hours exposure to the open air. This I have observed to be the case with the vitriolic acid air.

This air did not at all affect *salt-petre* or *borax*.

The acid air expelled from water saturated with the fluor acid, resembles the vitriolic acid air, in so many properties, that when I first published my experiments on the subject, I concluded it to be the vitriolic acid air combined with the fluor crust.

To satisfy myself with respect to it, I saturated a quantity of water with the fluor acid air, pressing out the stony matter with which it was filled at each process, and impregnating it over and over again. When it appeared to be sufficiently impregnated for my purpose, I put the liquor into a phial, furnished with a proper tube and recipient, such as is represented Plate II. fig. 8. to receive any of the watery part that might be expelled by heat; and applying the flame of a candle, I presently got from it great plenty of air; which, by every test that I could think of applying, appeared to have the very same properties with the vitriolic acid air.

The air thus expelled from this acid liquor was absorbed by water, without any crust on the surface of it.

When alkaline air was admitted to it, the sides of the vessel were tinged with the orange-colour mentioned above, which vanished in about an hour after it had been exposed to the open air.

This air had no effect upon *salt-petre*, a piece of which continued in it about a fortnight; nor yet upon *sulphur*, *alum*, or *sal ammoniac*.

Liver of Sulphur absorbed it, without undergoing any sensible change.

This air extinguished a candle, without any particular colour of the flame.

Camphor was dissolved in this air, exactly as it is in vitriolic acid air.

In these properties this acid air will be found, by comparison, to agree with the vitriolic acid air; as also in the two following, which, as far as I know, are peculiar to this species of air.

Phlogiston, as I have observed, is contained in vitriolic acid air, and in such a manner as to be communicated by it to the common air with which it is mixed, and thereby to phlogistificate, or injure it. And an equal quantity of this acid air, and common air, having been mixed, and left together twenty-four hours, the common air appeared to be so far injured, that two measures of it, and one of nitrous air, occupied the space of something more than two measures.

The *electric spark* has a very remarkable effect upon the vitriolic acid air, or rather upon the glass tube

tube in which the experiment is made, as has been observed; for a single explosion covers all the inside surface with a deep brown or black matter, and the glass grows more opaque every stroke. This very singular and striking effect has the electric spark taken in the air expelled from this acid liquor.

After I had made this experiment, I had no doubt, but that these two kinds of air, viz. the *vitriolic acid* and the *fluor acid*, are, in reality, the same. And the difference between them when the latter is divested of the stony matter which it contains, cannot be great. But still the acid thus far separated from the stony matter contains a sufficient quantity of it; for in making it boil with violence, the inside of the tube immediately connected with it, was filled with a stony matter. It happened twice in the course of the above mentioned experiments, that the tube was quite stopped up by this means, so as to cause the explosion of the phial.

Lastly, I would observe, that the *taste* of this acid liquor afforded a presumption, that the acid which enters into it was the vitriolic; for it has exactly the astringency of alum. Notwithstanding all these circumstances, the acid is unquestionably of a different nature.

That the fluor contains phlogiston, is evident, from the attempts that I made to procure dephlogisticated air from it, by means of spirit of nitre; for

the air that I got from it was always phlogisticated, and sometimes even nitrous.

At first I made this experiment by putting the materials into a phial with a ground-stopper and tube, and applying the heat of a candle only. The air I got in this manner neither affected common air, nor was affected by nitrous air. I then put the same apparatus into a crucible; and, with a strong sand-heat, I got from it about two ounce-measures of air, in four portions. The first of these was exactly like the preceding, being phlogisticated air; the second made lime-water turbid, and a great part of it was readily absorbed by water: the third and fourth portions were very strong nitrous air.

This experiment was made with the whitish part of this spar, which therefore probably contains the least phlogiston. That phlogiston which contributes to the *colour* of this fossil, I found, by the following observation, to be of a very volatile nature. When the coloured spar is dissolved in oil of vitriol, the fluor crust, collected in the water, has the same colour; but when it is dried near the fire, the colour vanishes, and the whole becomes white: yet this white crust, heated again in oil of vitriol, contains, as was observed before, so much phlogiston, as to convert oil of vitriol into vitriolic acid air.

The air expelled from this acid liquor did not dissolve the *fluor crust* that was exposed to it. A
quantity

quantity of it remained in this situation several days, without affecting it, or being affected by it. I had imagined that it might have been dissolved by this air, and have converted it into the fluor acid air.

Oil of turpentine absorbed about ten times its bulk of this air, and became of an orange colour. After this impregnation it had a pungent acid smell, together with its own. I observed nothing farther respecting it.

Reflecting upon the phosphoric property of the spar, by means of which I had procured this acid air, I thought it was possible that its property of enabling oil of vitriol to yield this air, might be common to it with other similar phosphoric substances, depending upon that combination of phlogiston which enables them to imbibe and emit light.

In order to ascertain this, with respect to one other substance of this kind, I made a quantity of Mr. *Canton's phosphorus*, and pouring upon it some oil of vitriol, I got air that was readily absorbed by water, and with a crust upon its surface, exactly like that which is procured from the fluor, only not in so great a quantity. The effervescence between this substance and the oil of vitriol was very great, and also the heat occasioned by it; and the vapour escaping into the common air, was white and dense, much like the vapour of the fluor acid.

I shall conclude this section with observing, that the oil of vitriol in which the fluor is dissolved,

becomes thick like ice, exactly like the oil of vitriol in which quick-lime has been boiled.

As the fluor contains an acid *sui generis*, it might have been expected, that it would have been dislodged from its base by some other of the acids, as well as the vitriolic; or that, if it was of a stronger kind than the nitrous, or marine acids, it might dislodge them from their bases: but no experiments whatever show any such thing; nor can the fluor acid be at all produced, except when the spar is dissolved in the vitriolic acid. This has, in some measure, been observed by others. Having carefully repeated the experiments myself, I observed the following results.

Having put a quantity of fluor to strong *nitrous acid*, in a glass phial with a ground stopper, I received the produce in water, and observed that about one sixth of it was fixed air, precipitating lime in lime-water, and that the remainder was strongly nitrous (a proof that this substance contains much phlogiston) but there was no appearance of any thing like a *crust* on the surface of the water. When the air was received in quicksilver, the solution of the metal, and the production of more nitrous air, demonstrated that the nitrous acid, uncombined with any thing that could alter the property of it, came over.

SECTION III.

Observations on the Freezing of Water, impregnated with Fluor Acid Air, and with Vitriolic Acid Air.

I Had an opportunity, in one frosty winter, of observing a pretty remarkable difference between water impregnated with vitriolic acid air, and water impregnated with the fluor acid. It is that water impregnated with vitriolic acid air that may be converted into ice, whereas water impregnated with fluor acid air will not freeze.

I had observed with respect to marine acid air, and alkaline air, that they dissolve ice, and that water impregnated with them is incapable of freezing, at least in such a degree of cold as I had exposed them to. The same, I find, is the case with fluor acid air, but it is not so at all with vitriolic acid air, which, intirely contrary to my expectation, I find to be altogether different from marine acid air in this respect, and to resemble fixed air. But whereas water impregnated with fixed air discharges it when it is converted into ice, water impregnated with vitriolic acid air, and then frozen, retains it as strongly as ever.

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I exposed

I exposed in an open phial a quantity of water fully impregnated with vitriolic acid air, when the thermometer was at 17 degrees, and observed that it was presently frozen quite through, the smell of it continuing to be extremely pungent. As it melted, the ice sunk to the bottom of the liquor, and when it was quite dissolved, the water appeared to be still very strongly impregnated,

Then, in order to try whether water would freeze in a situation in which none of this acid air could possibly escape from it, I exposed to the same degree of cold a quantity of water saturated with this air, remaining upon the quicksilver, in the jar in which the impregnation had been made; but this water also was presently converted into ice, which was opaque, though the bubbles were not distinctly perceivable.

I then poured a quantity of *water* upon a quantity of the ice above mentioned, when it froze, without being seemingly affected by any effluvium of the impregnated ice. This continued opaque, while that of the water was transparent as usual. Both these kinds of ice melted equally, and adhered together.

Ice was not at all dissolved in vitriolic acid air. Letting it remain in this situation all night, the next morning I found all the air absorbed; but it appeared, by the form of the ice, that it had been melted,
and

and frozen again ; for it exactly fitted the glass vessel in which it was confined.

Ice was readily dissolved at first in fluor acid air, but afterwards very slowly, as might have been concluded from the manner in which this kind of air is imbibed by water. However, in time, the whole of the air was absorbed by the ice, which was dissolved by it, and continued fluid on the surface of the quicksilver.

In order to make my experiment upon water impregnated with fluor acid air, I saturated a quantity of water with it so fully, that I could with difficulty squeeze any water from a quantity of fluor crust that was deposited in it. This water I exposed to the frost all night, and in the morning found it to be entirely fluid. Presently after this the frost broke, and I had no opportunity of repeating the experiment.

The farther prosecution of this experiment, and a proper attention to it, will probably throw great light on the nature of freezing.

The frost of a subsequent winter confirmed the above mentioned observations, except those which respected water impregnated with fluor acid air, which I before concluded would not freeze.

I now find that it does freeze, though it requires a greater degree of cold than water impregnated with
vitriolic

vitriolic acid air. The latter effect I attributed to the presence of some of the fluor crust in the solution, and I think this conjecture is, in some measure, confirmed by the following observations; in which it will be seen, that lime water did not freeze so soon as common water, and that lime water impregnated with vitriolic acid air did not freeze so soon as common water so impregnated.

Jan. 7, 1779. I exposed to the cold all night a phial of pump water, and one of the same water saturated with quick lime. The next morning I found the thermometer at 28, the pump water frozen solid, but the lime water not frozen at all.

Jan. 9. When the thermometer was at least 23 water impregnated with fluor acid air, after being exposed to the cold all night, was imperfectly frozen. At the same time water impregnated with vitriolic acid air was quite solid, and also a quantity of the same in which some chalk had been dissolved. But lime impregnated with vitriolic acid air was quite fluid. Lime water was frozen, and a little of the lime was precipitated.

Jan. 12. When the thermometer was at 20, and had probably been lower in the night, I found the lime water impregnated with vitriolic acid air, and also the water impregnated with fluor acid air, solid

solid throughout. The former was quite white, but was transparent again when the ice melted. As the ice of the fluor acid melted, it swam on the surface of the liquid part.

SECTION IV.

Miscellaneous Experiments on the Properties of Fluor Acid Air.

DIPPING a lighted candle into a vessel filled with the fluor acid air, it was extinguished without any particular colour of the flame, which is observable in the marine acid air.

Nitrous air, mixed with this acid air, had no sensible effect upon it. Water absorbed the acid air, and left the nitrous air possessed of its peculiar properties.

Having

Having ascertained the effect of water upon this acid air, I proceeded to try other *fluid substances*.

Spirit of wine imbibed this air as readily as water, but continued as limpid as ever; and when saturated with it, seemed to be no less inflammable than before.

Oil of turpentine did not imbibe any of this air.

Vitriolic ether imbibed about twenty times its own bulk of it; but was not sensibly changed by the impregnation. The case was the same with *nitrous ether*. But the first time that I made the experiment with nitrous ether, I imagine a little water was mixed with it (as much as those substances are capable of being mixed) for it coagulated as water had done, remaining in the middle of the tube, the acid air being both above and below it. This mass of coagulated matter, which in colour and consistence resembled a brown jelly, being taken out of the vessel, did not take fire at the approach of a candle; but when it had been exposed to the air about half a minute, it grew hot, threw out a gross smoke, and was presently all evaporated. Part, however, of the same mass, which had been dipped in water, did not grow hot, or evaporate, in the open air; and when exposed to the fire, it burned to a white powdery substance. I imagine this effect to have been owing to a mixture of water; because, with
4 pure

pure nitrous ether, I could not get another appearance of the kind.

Of *solid substances*, I found that this air had no effect upon *sulphur*, *common salt*, *sal ammoniac*, *iron*, *liver of sulphur*, or *gum-lac*.

Charcoal absorbed the whole of a quantity of this air, and contracted from it a strong pungent smell. The *rust of iron* also absorbed it in like manner.

Alum absorbed this air pretty fast, the surface of it being rendered white and opake. When it was taken out of the air, it looked moist, and was incapable of the operation of roasting, like that which had been exposed to alkaline air. This air having, no doubt, like the other, seized upon the water which enters into the composition of alum.

Quicklime and *chalk*, both absorbed a little of this acid air; but the result was, in no respect, remarkable. The latter had been dissolved by it, and had produced a quantity of fixed air, precipitating lime in lime-water.

In order to judge whether there was any foundation for the opinion of Mr. Boulanger, of this acid being the same with the *marine*, I put to it a piece of salt-petre, which I have observed to be readily dissolved in the marine acid air; and I must own that appearances so much favoured his opinion, that I was at that time very much inclined to adopt it.

When

When the salt-petre had been for some time surrounded with this air, the air began to be diminished, and the inside of the vessel was filled with red fumes, which continued about a week, the quicksilver rising all the time, till only one tenth of the air remained, and the inside of the vessel was covered with a whitish, probably a saline, substance, produced by the solution of mercury. After this, the air becoming transparent, I examined it, and found it neither to affect common air, nor to be affected by nitrous air, and to extinguish a candle. Also, about one fourth of it was readily absorbed by water, and made lime water turbid; so that, contrary to my expectation, a great part of the air must have been fixed air, and not nitrous. This experiment I did not repeat; but it seems to exhibit a fact deserving of some attention.

Fluor acid air, when it is first produced, corrodes the glass vessel in which it is generated. But whether it did this of itself, merely in consequence of being heated, or whether the moisture, or something else contained in the oil of vitriol, by means of which it is formed, contributed to this effect, did not certainly appear. When this air is cold, it does not at all affect the glass vessel in which it is confined. In my attempts to confine the different kinds of air in glass tubes hermetically sealed,
in

in order to expose them to a continued heat, I observed that it is simply the *beated air* that has this effect. For when I had filled a tube with this kind of air, and was endeavouring to take off different lengths of it with a blow pipe, I found that, when the glass became red hot, it was always so corroded, and dissolved, that it was impossible to close it by sealing.

Having mentioned the effect of heating various substances in different kinds of air by means of a burning lens, that I may not omit to mention any thing that I have observed of the kind, and that I can think any person might wish to be informed of, I shall, in this miscellaneous section, recite the following experiments.

Throwing the focus of the lens upon iron turnings in fluor acid air, a dense white fume presently filled the vessel, and during the process the heat was remarkably great, so that I could not touch the upper part of the glass vessel in which the experiment was made. The quantity of air was considerably diminished, and a quantity remained that was not absorbed by water, but not enough to examine it.

P A R T IV.

EXPERIMENTS AND OBSERVATIONS RELATING TO
ALKALINE AIR:

SECTION I.

*The Discovery of Alkaline Air, and of the Impregnation
of Water with it.*

AFTER I had made the discovery of the *marine acid air*, which the vapour of spirit of salt may properly enough be called, and had made those experiments upon it, of which I have already given an account, it occurred to me, that, by a process similar to that by which this *acid air* is expelled from the spirit of salt, an *alkaline air* might be expelled from substances containing volatile alkali.

Ac-

Accordingly I procured some volatile spirit of sal ammoniac, and having put it into a thin phial, and heated it with the flame of a candle, I presently found that a great quantity of vapour was discharged from it; and being received in a vessel of quicksilver, standing in a basin of quicksilver, it continued in the form of a transparent and permanent air, not at all condensed by cold; so that I had the same opportunity of making experiments upon it, as I had before on the acid air, being in the same favourable circumstances.

With the same ease I also procured this air from *spirit of hartshorn*, and *sal volatile* either in a fluid or solid form, *i. e.* from those volatile alkaline salts which are produced by the distillation of sal ammoniac with fixed alkalis. But in this case I soon found that the alkaline air I procured was not pure; for the fixed air, which entered into the composition of my materials, was expelled along with it. Also, uniting again with the alkaline air, in the glass tube through which they were conveyed, they stopped it up, and were often the means of bursting my vessels.

While these experiments were new to me, I imagined that I was able to procure this air with peculiar advantage and in the greatest abundance, either from the salts in a dry state, when they were just covered with water, or in a perfectly fluid

state; for, upon applying a candle to the phials in which they were contained, there was a most astonishing production of air; but having examined it, I found it to be chiefly fixed air, especially after the first or second produce from the same materials; and removing my apparatus to a trough of water, and using the water instead of quicksilver, I found that it was not presently absorbed by it.

This, however, appears to be an easy and elegant method of procuring fixed air, from a small quantity of materials, though there must be a mixture of alkaline air along with it; as it is by means of its combination with this principle only, that it is possible, that so much fixed air should be retained in any liquid. Water, at least, we know, cannot be made to contain much more than its own bulk of fixed air.

After this disappointment, I confined myself to the use of that volatile spirit of sal ammoniac which is procured by a distillation with flaked lime, which contains no fixed air; and which seems, in a general state, to contain about as much alkaline air, as an equal quantity of spirit of salt contains of the marine acid air.

Wanting, however, to procure this air in greater quantities, and this method being rather expensive, it occurred to me, that alkaline air might, probably,

be procured, with the most ease and convenience, from the original materials, mixed in the same proportions that chemists had found by experience to answer the best for the production of the volatile spirit of sal ammoniac. Accordingly I mixed one fourth of pounded sal ammoniac, with three fourths of slaked lime; and filling a phial with the mixture, I presently found it completely answered my purpose. The heat of a candle expelled from this mixture a prodigious quantity of alkaline air; and the same materials (as much as filled an ounce phial) would serve me a considerable time, without changing; especially when, instead of a glass phial, I made use of a small iron tube, which I find much more convenient for the purpose.

As water soon begins to rise in this process, it is necessary, if the air is intended to be conveyed perfectly *dry* into the vessel of quicksilver, to have a small vessel in which this water (which is the common volatile spirit of sal ammoniac) may be received. This small vessel must be interposed between the vessel which contains the materials for the generation of the air, and that in which it is to be received, as *d*, Pl. II. fig. 8.

This *alkaline* air being perfectly analogous to the *acid* air, I was naturally led to investigate the properties of it in the same manner, and nearly in the same order. From this analogy I concluded,

as I presently found to be the fact, that this alkaline air would be readily imbibed by water, and, by its union with it, would form a volatile spirit of sal ammoniac. And as the water, when admitted to the air in this manner, confined by quicksilver, has an opportunity of fully saturating itself with the alkaline vapour, it is made prodigiously stronger than any volatile spirit of sal ammoniac that I have ever seen; and I believe stronger than it can be made in the common way.

In order to ascertain what addition, with respect to quantity and weight, water would acquire by being saturated with alkaline air, I put one grain and a quarter of rain water into a small glass tube, closed at one end with cement, and open at the other, the column of water measuring seven tenths of an inch; and having introduced it through the quicksilver into a vessel containing alkaline air, observed that it absorbed seven eighths of an ounce measure of the *air*, and had then gained about half a grain in weight, and was increased to eight tenths of an inch and a half in length. I did not make a second experiment of this kind, and therefore will not answer for the exactness of these proportions in future trials. What I did sufficiently answered my purpose, in a general view of the subject.

When I had, at one time, saturated a quantity of distilled water with alkaline air, so that a good
deal

deal of the air remained unabsorbed on the surface of the water, I observed that, as I continued to throw up more air, a considerable proportion of it was imbibed, but not the whole; and when I had let the apparatus stand a day, much more of the air that lay on the surface was imbibed. And after the water would imbibe no more of the *old* air, it imbibed *new*. This shews that water requires a considerable time to saturate itself with this kind of air, and that part of it more readily unites with water than the rest.

The same is also, probably, the case with all the kinds of air with which water can be impregnated. Mr. Cavendish made this observation with respect to fixed air, and I repeated the whole process above-mentioned with marine acid air, and had precisely the same result. The alkaline water which I procured in this experiment was, beyond comparison, stronger to the smell, than any spirit of sal ammoniac that I had seen.

This experiment led me to attempt the making of spirit of sal ammoniac in a larger quantity, by impregnating distilled water with this alkaline air. For this purpose I filled a piece of a gun barrel with the materials above-mentioned, and luted to the open end of it a small glass tube, one end of which was bent, and put within the mouth of a glass vessel, containing a quantity of distilled water

upon quicksilver, standing in a basin of quicksilver. In these circumstances the heat of the fire, applied gradually, expelled the alkaline air, which, passing through the tube, and the quicksilver, came at last to the water, which, in time, became fully saturated with it.

By this means I got a very strong alkaline liquor, from which I could again expel the alkaline air which I had put into it, whenever it happened to be more convenient to me to get it in that manner. This process may easily be performed in a still larger way; and by this means a liquor of the same nature with the volatile spirit of sal ammoniac, might be made much stronger, and much cheaper, than it is now made.

I had some expectation that alkaline air might be expelled from caustic *fixed alkali*, especially as it is known that the fixed and volatile alkalis differ only in their combinations; but I was disappointed in my expectations. Having procured a quantity of caustic alkali from Mr. Lane, who is known to prepare it with particular accuracy, I treated it in the same manner as I had done the spirit of salt, and found that the vapour expelled from it consisted of nothing but water, being immediately condensed when it came to the cold quicksilver,

SECTION II.

*The Mixture of Alkaline Air with various Substances,
and miscellaneous Properties of it.*

HAVING satisfied myself with respect to the relation that alkaline air bears to water, I was impatient to find what would be the consequence of mixing this new air with the other kinds with which I was acquainted before, and especially with marine *acid* air; having a notion that these two airs, being of opposite natures, might compose a *neutral air*, and perhaps the very same thing with common air. But the moment that these two kinds of air came into contact, a beautiful white cloud was formed, and presently filled the whole vessel in which they were contained. At the same time the quantity of air began to diminish, and, at length, when the cloud was subsided, there appeared to be formed a solid *white salt*, which was found to be the common *sal ammoniac*, or the marine acid united to the volatile alkali.

The first quantity that I produced immediately deliquesced, upon being exposed to the common

air; but if it was exposed in a very dry and warm place, it almost all evaporated, in a white cloud. I have, however, since, from the same materials, produced the salt above-mentioned in a state not subject to deliquesce, or evaporate. This difference, I find, is owing to the proportion of the two kinds of air in the compound. It is only volatile when there is more than a due proportion of either of the constituent parts. In these cases the smell of the salts is extremely pungent, but very different from one another; being manifestly acid, or alkaline, according to the prevalence of each of these airs respectively.

Nitrous air admitted to alkaline air likewise occasioned a whitish cloud, and part of the air was absorbed; but it presently grew clear again; leaving only a little dimness on the sides of the vessel. This, however, might be a kind of salt, formed by the union of the two kinds of air. There was no other salt formed that I could perceive. Water being admitted to this mixture of nitrous and alkaline air presently absorbed the latter, and left the former possessed of its peculiar properties.

Fixed air admitted to alkaline air formed oblong and slender crystals, which crossed one another, and covered the sides of the vessel in the form of net-work. These crystals must be the same thing with the volatile alkalis which chemists get in a solid

solid form, by the distillation of sal ammoniac with fixed alkaline salts.

Inflammable air admitted to alkaline air exhibited no particular appearance. Water, as in the former experiment, absorbed the alkaline air, and left the inflammable air as it was before. It was remarkable, however, that the water which was admitted to them became whitish, and that this white cloud settled, in the form of a white powder, to the bottom of the vessel.

Alkaline air mixed with *common air*, and standing together several days, first in quicksilver, and then in water (which absorbed the alkaline air) it did not appear that there was any change produced in the common air; at least it was as much diminished by nitrous air as before. The same was the case with a mixture of marine acid air and common air.

Having mixed air that had been diminished by the fermentation of a mixture of iron filings and sulphur with alkaline air, the water absorbed the latter, but left the former, with respect to the test of nitrous air (and therefore, as I conclude, with respect to all its properties) the same that it was before.

Spirit of wine imbibes alkaline air as readily as water, and seems to be as inflammable afterwards as before,

Alkaline

Alkaline air contracts no union with *olive oil*. They were in contact almost two days, without any diminution of the air. Oil of turpentine, and essential oil of mint, absorbed a very small quantity of alkaline air, but were not sensibly changed by it.

Ether, however, imbibed alkaline air pretty freely; but it was afterwards as inflammable as before, and the colour was not changed. It also evaporated as before, but I did not attend to this last circumstance very accurately.

Sulphur, nitre, common salt, and flints, were put to alkaline air without imbibing any part of it; but *charcoal, sponge, bits of linen cloth*, and other substances of that nature, seemed to condense this air upon their surfaces; for it began to diminish immediately upon their being put to it; and when they were taken out the alkaline smell they had contracted was so pungent as to be almost intolerable, especially that of the sponge. Perhaps it might be of use to recover persons from swooning. A bit of sponge, about as big as a hazel nut, presently imbibed an ounce measure of alkaline air.

A piece of the inspissated juice of *turnsole* was made very dry and warm, and yet it imbibed a great quantity of the air; by which it contracted a most pungent smell, but the colour of it was not changed.

Alum

Alum undergoes a very remarkable change by the action of alkaline air. The outward shape and size remain the same, but the internal structure is quite changed, becoming opaque, beautifully white, and, to appearance, in all respects like alum which had been roasted; and so as not to be at all affected by a degree of heat that would have reduced it to that state by roasting. This effect is produced slowly; and if a piece of alum be taken out of alkaline air before the operation is over, the inside will be transparent, and the outside, to an equal thickness, will be a white crust.

I imagine that the alkaline vapour seizes upon the water that enters into the constitution of crude alum, and which would have been expelled by heat. Roasted alum also imbibes alkaline air, and, like the raw alum that has been exposed to it, acquires a taste that is peculiarly disagreeable.

Phosphorus gave no light in alkaline air, and made no lasting change in its dimensions. It varied, indeed, a little, being sometimes increased and sometimes diminished, but after a day and a night, it was in the same state as at the first. Water absorbed this air just as if nothing had been put to it.

In some chemical processes, volatile alkali dissolves copper. This I also have observed in my
account

account of the experiments in which I put some pieces of volatile alkaline salt to a quantity of common air, at the time that I introduced nitrous air to it. For, if the alkaline salt be supported by copper wire, it presently becomes blue, and is soon corroded. I therefore thought that pieces of copper, exposed to pure *alkaline air*, would have been affected in the same manner; but I did not find this to be the case. A number of pieces of copper wire remained a whole night in alkaline air without sensibly affecting it, or being affected by it. That the alkaline air was pure, appeared by its being wholly absorbed by water afterwards.

Having put some *spirit of salt* to alkaline air, the air was presently absorbed, and a little of the white salt above-mentioned was formed. A little remained unabsorbed, and transparent, but upon the admission of common air to it, it instantly became white.

Oil of vitriol, also formed a white salt with alkaline air, and this did not rise in white fumes.

Marine acid air, as I have observed, extinguishes a candle. Alkaline air, on the contrary, I was surprized to find, is slightly inflammable; which, however, seems to confirm the opinion of chemists, that the volatile alkali contains phlogiston.

I dipped

I dipped a lighted candle into a tall cylindrical vessel, filled with alkaline air, when it went out three or four times successively; but at each time the flame was considerably enlarged, by the addition of another flame, of a pale yellow colour; and at the last time this light flame descended from the top of the vessel to the bottom. At another time, upon presenting a lighted candle to the mouth of the same vessel, filled with the same kind of air, the yellowish flame ascended two inches higher than the flame of the candle. The electric spark taken in alkaline air is red, as it is in common inflammable air.

Though alkaline air be inflammable, it appeared, by the following experiment, to be heavier than the common inflammable air, as well as to contract no union with it. Into a vessel containing a quantity of inflammable air, I put half as much alkaline air, and then about the same quantity of marine acid air. These immediately formed a white cloud, but it did not rise within the space that was occupied by the inflammable air; so that this latter had kept its place above the alkaline air, and had not mixed with it.

That alkaline air is lighter than acid air is evident from the appearances that attend the mixture, which are indeed very beautiful. When acid air

is introduced into a vessel containing alkaline air, the white cloud which they form appears at the bottom only, and ascends gradually. But when the alkaline air is put to the acid, the whole becomes immediately cloudy, quite to the top of the vessel.

In the last place, I shall observe that alkaline air, as well as acid, dissolves *ice* as fast as a hot fire can do it. This was tried when both the kinds of air, and every instrument made use of in the experiment, had been exposed to a pretty intense frost several hours. In both cases, also, the water into which the ice was melted dissolved more ice, to a considerable quantity.

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SECTION III.

Of the Mixture of all the Kinds of Acid Air with Alkaline Air.

THAT the fluor acid air saturates the same quantity of *alkaline air* with vitriolic acid air, appeared in a course of experiments not made with this particular view, but designed to ascertain the quantity of acid in all the kinds of acid air. I first thought of attempting this by finding what quantity of water, tinged blue with the juice of turnsole, an equal quantity of each of the kinds of air would turn red. But it could not have been determined with so much accuracy in this way, at least not so *visibly*, and demonstrably, as by the saturation of them with alkaline air; *diminution of bulk* being a thing easily measured. The result of these experiments is, in several respects, rather extraordinary; being such as, I imagine, no person would have conjectured *a priori*. At least it was contrary to my expectations. As I made these experiments in two different ways, and the results, though agreeing in general,

general, differ a little, it may be proper to recite the particulars of both.

In order to determine what quantity of acid and alkaline air would saturate one another, I first filled a glass jar with quicksilver, and put into one measure (which was about an ounce measure and a half) of alkaline air, and then the same quantity (being from the very same phial) of *marine acid air*; when the whole of both very nearly disappeared, a quantity of sal ammoniac being formed. I then threw into the same jar another measure of *marine acid air*, and observed that it occupied about one sixth less space than the original measure of alkaline air; so that a little more of the acid than of the alkaline air had disappeared. I then threw in the same measure of each of the kinds of air alternately, till I had, in all, five measures of each; after which hardly any air remained, except what was not absorbed by water, which was about a fourth part of the original measure, and was very little different from common air; two measures of it and one of nitrous air occupying the space of one measure and a quarter. Upon the whole, therefore, it may safely be concluded from this experiment, that equal quantities of these two kinds of air saturate one another.

When

When I put a measure of *vitriolic acid air* to the alkaline air, I found that a given quantity of the latter was saturated, as nearly as I could judge, by half as much of the former. For when they were mixed in that proportion, the whole quantity of both kinds of air entirely disappeared. It was in making this experiment, always throwing in two measures of alkaline air for one of the vitriolic, that I got the thick *yellow coating* of the inside of the jar mentioned above, and which became white by being exposed to the open air.

Making the experiment in the same manner, I also found that a measure of alkaline air was completely saturated by half a measure of *fluor acid air*, so that both intirely disappeared. In this remarkable circumstance the fluor acid air and vitrollic acid air perfectly agree; which appeared to me to afford a very strong argument of their ultimate identity.

Having put one measure of *fixed air* to one of alkaline air, the quicksilver rose to half a measure; and when I introduced another measure of fixed air, no more was absorbed; and water being admitted to it, imbibed almost all the remainder; so that half a measure of fixed air had saturated a whole measure of alkaline air. But the union of these two kinds of air is not effected so quickly as in the other cases; otherwise the whole of the alkaline air must have disappeared, when half the quantity of fixed

air had been admitted to it. Afterwards, making this mixture very gradually, one third of the quantity of fixed air saturated almost the whole of the alkaline air.

Lastly, I filled the vessel in which I had measured the preceding kinds of air, with *red vapour of spirit of nitre*, and put to it an equal quantity, and one fourth more, of alkaline air; when it immediately became very turbid and white, and the quicksilver rose within the vessel. Then filling the same phial again with the red vapour, as nearly as I could in the same manner, and admitting water to it, one fourth of the contents of the phial was absorbed. This, therefore, must have been the space that would have been occupied by the pure nitrous vapour, if there had been no common air mixed with it. Consequently one measure of nitrous vapour, in fact, saturated about five measures of alkaline air. But there is a considerable degree of uncertainty with respect to the real quantity of nitrous vapour in a phial that is seemingly filled with it.

In the next set of experiments, without taking equal measures both of alkaline and of the several kinds of acid air, I took equal measures of the acid airs only; and having a large jar of alkaline air, I endeavoured to find how much of it equal quantities of the several acid airs would saturate. The vessel, which in all these cases I filled with acid air, contained

contained an ounce measure and one sixth, and the diminution of bulk in the jar of alkaline air (which was carefully filled a new in all the trials) by the several kinds of acid air was, as nearly as possible, in the following proportion :

A measure of fluor acid air, }	
absoberd of the alkaline air }	$1 \frac{1}{6}$
<u>vitriolic</u>	2
<u>marine</u>	$1 \frac{1}{8}$
<u>fixed air</u>	$1 \frac{1}{4}$

Had I mixed the fixed air very slowly, as in one of the cases of the preceding set of experiments, more of the alkaline air would probably have been saturated with it.

I would observe also, that, in one respect, this latter method of making the experiments, is less accurate than the other; as I did not make allowance for the different degrees of dilatation of the air above the quicksilver by the weight of the quicksilver in the jar above the level of the quicksilver without the jar. But to have avoided this inaccuracy, I must have had a deeper vessel, and more quicksilver than I happened to have at hand. But the results of the experiments, in both the methods of making them, agree so very nearly, that I have

little doubt but that the conclusions may be sufficiently depended upon.

That vitriolic acid air should saturate more alkaline air than marine acid air can do, is agreeable enough to the observation of the quantities of *water* they respectively saturate; the difference between them, though much greater in this case, being *in the same way* as in the other. For an equal quantity of water will imbibe ten times as much marine acid air as of vitriolic. Water will also take little more than its bulk of fixed air; so that a given quantity of alkaline air ought, for the same reason, to take less of this, than of any other kind of acid air, which agrees with the experiment. And yet, considering the apparent *strength* of the acids, one would imagine that the vitriolic, which, in this form, is weaker than the marine, should saturate less of the alkaline air; and again that the fixed air, being the weakest of all the acids, should saturate the least quantity of alkaline air. But the former view, in which the experiments do not appear at all extraordinary, is, in fact, more agreeable to the analogy of the saturation of water with these acids; and the difference in the proportion absorbed of each of them, corresponds to the difference of the specific gravities of water and air. But I did not see the subject in this light *a priori*.

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SECTION IV.

Of the electric Spark in Alkaline Air, and its Conversion into inflammable Air.

I Took the electric explosion in a small quantity of *alkaline air*, and observed, that every stroke added considerably to the quantity of air; and when water was admitted to it, just so much remained unabsorbed as had been added by the explosions. I then took about an hundred explosions of the same jar, in a larger quantity of alkaline air; after which, so much of it remained unabsorbed by water, that I could examine it with the greatest certainty. It neither affected common air, nor was affected by nitrous air, and was as strongly inflammable as any air that I had ever procured.

In the next place, I ascertained the *quantity* of inflammable air that may be produced from any given quantity of alkaline air. And this production having its limits, certainly shews that the alkaline air supplies some essential part in the constitution of it.

To take my measures with more accuracy, I, at this time, confined the alkaline air in a glass tube, of the same dimensions throughout; and having it confined, as it necessarily must be, by quicksilver, I carefully marked the space which it occupied in the tube. I then took the electric spark, or explosion, which ever of them happened to be the most convenient, till I perceived that no more addition was made to the quantity of air; and then measuring the space which it occupied, I found that the whole was, as nearly as possible, three times as much as that which the alkaline air alone had occupied. When I examined this air, I found it to have an inflammability of the strongest kind, firing with explosions, and in no respect to be distinguished from that which is extracted from metals by acids. Also the electric spark taken in this air was always red, though, as is also the case with other inflammable air, it was white in the centre of any considerable explosion taken in it.

After this experiment it still remained a doubt whether, when the process was completed, there did not remain, at least, some portion of alkaline air not affected by it, and capable of being absorbed by water afterwards. To determine this, and likewise to repeat so important an experiment upon a larger scale, I began with one third of an ounce measure of alkaline air, and I took the electric spark

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in it till I had got a complete ounce measure of air. Then admitting a little water to it, I observed with the greatest attention, but could not perceive that any part of the air was absorbed by it. However, when I had made this air explode, by means of the flame of a candle, and immediately after applied my nostrils to the mouth of the vessel in which it had been contained, I perceived a very evident alkaline smell; so that the whole of the volatile alkali had not been completely incorporated with this air, though it was so much so, as not to be seized by the *water*. And to give it a fairer trial, this water had been confined along with the air, upon the quicksilver, and had been even frequently agitated with it, during two whole days; and though it was but a very small quantity, it had no perceivable alkaline smell afterwards.

When I reflected upon this change of the alkaline air by the electric explosion, I considered that it might be produced either by *electricity as such*, or by the *light*, or the *heat*, which accompany the experiment. In order to try the effect of *light*, I exposed a small quantity of alkaline air to a strong light made by a burning lens, for several hours. But this made no sensible change in the quantity of it. I therefore concluded that the proper agent in this process was not light.

I did not immediately proceed to try the effect of heat, but observed it by accident some time afterwards, in the following manner. At the time that I was reviving metals in inflammable and alkaline air, I put a quantity of ochre into the latter, expecting to revive it in the form of iron; and in this I seemed to succeed, as the ochre became black in the course of the process. But what struck me the most was, that instead of perceiving the quantity of air to decrease, as I had expected, there was very soon a visible increase of it. Examining the air, I found it to contain no fixed air, and to be all strongly inflammable.

Still attending to other circumstances besides the change of the alkaline air, I proceeded to heat in it scales of iron, and iron itself, of different kinds, and still found that the air was increased in quantity, and changed in its quality. But in this way I could never increase the bulk of the air more than twice, and seldom quite so much. Also, after the process, when I could make no farther change in the air, I still perceived a very strong smell of volatile alkali. The iron had undergone no change that I could discover, so that I concluded that the air had been affected by means of *heat* only.

To ascertain this, I then proceeded to heat in the alkaline air bits of dry crucibles, or of earthen retorts, which had been just before exposed to very
great

great heats, so that they could not be supposed to give out any air in this process, and therefore could only serve to produce a strong heat in the alkaline air. The result seemed to confirm my supposition. For in these experiments with earthy substances the effect was always the same as when I had made use of ochre or iron. The air was increased in bulk, but the quantity not quite doubled, and it was become inflammable.

The bits of *white* earthen ware having always become *black* in this process, I suspected that this kind of substance might have affected the air in some other way than by merely communicating heat to it. To try this, I used the same small bit of crucible again and again, and finding the same effect from heating it at the last, as at the first, I could not help concluding that the change produced in the air had been effected by means of *heat* only.

In all these experiments, however, there was a strong *light* as well as an intense heat concerned. In order to ascertain, therefore, whether the light, at least in conjunction with heat, might not have had some effect in this change of the air, I heated a quantity of it in a coated green glass retort. The mouth of the retort was plunged in a basin of mercury, and I received in a glass tube filled with

with water all the air that was driven out of the retort by heat. At first it was all absorbed by the water; being merely alkaline air expelled by rarefaction. But when I concluded that the bulb of the retort was become red hot, the bubbles that were driven out were not wholly absorbed by water, and by degrees no part of them was absorbed. This air I examined, and found it to be inflammable. The retort was melted in the process, so that I could not examine the air that remained in it; but my purpose was sufficiently answered, as I saw no room to doubt, but that the change of the alkaline into inflammable air was produced by *mere heat*, without the aid of light, if a red heat can be so termed. How the heat operates in this case I cannot tell.

It was observable, that whenever I changed alkaline air into inflammable, by heating in it bits of crucibles or retorts, into the composition of which *clay* enters, they always became black. There was room, therefore, to suppose that it was not by means of *mere heat* that this change was made, but that something might be deposited from the air, which might attach itself to the clay. Indeed, if this was not the case, I do not see why the clay should become black; though, perhaps, part of the same phlogiston which forms the inflammable
air

air may be attracted by the red hot clay, without there being any proper decomposition of the air.

That this is the case, seems probable from an experiment in which I used white *porcelain*, instead of bits of earthen ware, into the composition of which clay enters. For this substance did not become black in the process, and yet inflammable air was produced. But I must observe, that a considerable *time* was requisite to produce this effect in these circumstances, and that it was not much inflammable air that I was able to procure in this way. It was, however, in winter, and the heat that I could apply was not great.

Alkaline air is converted into inflammable air by making it pass through a red hot earthen tube, as well as by the electric spark, but by no means, I think, in so great a degree. I put two ounces and ten pennyweights of water, pretty strongly impregnated with alkaline air, into the retort, and heating it, sent the vapour through the hot tube; when I collected two ounces three pennyweights of liquor, which had a disagreeable empyreumatic smell, as well as that of volatile alkali; and it was quite opake with a black matter which subsided to the bottom of the vessel. Also the tube through which the air and vapour had been conveyed was left quite black.

One of the junctures in the apparatus not having been air tight, I did not collect all the air; but it came only at the beginning of the process, and before the tube became black, or any liquor was distilled; and it was all strongly inflammable.

SECTION V.

The Analysis of Alkaline Air.

THAT alkaline air contains phlogiston appears both by the calces of metals being revived in it, and by its being converted into a species of inflammable air, by the electric explosion, and other processes. By the following experiments it seems to contain less phlogiston than an equal bulk of inflammable air from iron, and about one fourth of its bulk of phlogisticated air. But it is not possible to ascertain this with perfect exactness, from the nature of the processes. I shall therefore recite the experiments in the order in which they were made, some of them being more, and others less accurate.

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I first observed that some *massicot*, which I used in some of these experiments, contained a portion of fixed air, which could be expelled from it by heat; but when I melted it in alkaline air, by which means the lead was always revived, I found no fixed air in the residuum, but only pure phlogisticated air. This, however, is easily accounted for, as the alkaline air would immediately unite with any fixed air that should be set loose in the process.

With respect to the quantity of lead revived in alkaline air, it is difficult to be ascertained, on two accounts. In the first place, some of the calx is blackened, and imperfectly revived; and again, if particular care be not taken, the lead that is revived will be dissolved in the mercury, by which the air must necessarily be confined. To prevent this last inconvenience, I put the powdered *massicot* into small earthen cups, which I contrived to place with their mouths upwards, so that when the lead was revived, by means of a burning lens, it would remain in the cup, and not mix with the mercury by which it was supported. On these accounts, however, the largest quantity of lead that was revived must always be considered as nearest the truth.

In three ounce measures of alkaline air, I first revived six grains of lead, then in three ounces and a half

a half sixteen grains, in two ounces and a half thirteen grains, and in three ounces three fourths twelve grains. In five ounce measures I revived ten grains, besides blackening a quantity of it, and there remained one ounce measure of phlogisticated air. But the experiment which I made with the most care, and on which I place the greatest dependence, was the following.

In seven ounce measures and a half of alkaline air, I revived twenty six grains and a half of lead, and there remained two ounce measures and a half of phlogisticated air. In this proportion 100 ounce measures of alkaline air would revive 352 grains of lead; whereas 100 ounce measures of inflammable air from iron would revive 480 grains of lead. But alkaline air is resolvible into considerably more than twice its bulk of inflammable air, and therefore it might have been expected that it would have revived much more lead. Would it not, therefore follow from this, that phlogisticated air contains a very great quantity of phlogiston, many times more than an equal bulk of pure inflammable air. Supposing alkaline air to be resolvible into no more than twice its bulk of inflammable air, a hundred ounce measures of it ought to revive 960 grains of lead. But in the proportion mentioned above, it would revive no more than 608 grains. As much phlogiston, therefore,

fore, as would revive 342 grains of lead (which is the difference between these two numbers) should be contained in about twenty five ounce measures of phlogisticated air. Consequently a hundred ounce measures of phlogisticated air would revive 1368 grains of lead, which is more than twice as much as an equal quantity of pure inflammable air would revive. It must, however, be considered, that the inflammable air, into which alkaline air is resolvable, is not the same with that which is procured from iron, and may contain much less phlogiston.

To these experiments, on the revival of lead in alkaline air, I shall subjoin an account of some that were made with other calces, heating them all, when confined by this air, by means of a burning lens.

Glass of antimony absorbed a part of the alkaline air, and there was the appearance of metal in several small globules, whereas I could get no such appearance from the same substance in inflammable air.

By heating scales of iron in alkaline air, it was much increased in quantity, and then I found it to contain no fixed air, but the whole was strongly inflammable. No water was produced in this process. The iron was revived, being perfectly soluble in diluted oil of vitriol; and from being twenty
four

four grains had lost one grain in weight. It is evident from this experiment, that there is water in alkaline air.

As alkaline air evidently contains phlogiston, and as such was capable of reviving lead, and no doubt the other metals, I had the curiosity to make use of it in other circumstances in which I had before used inflammable air; and among other things I tried the effect of heating red precipitate in it. The consequence was, that the mercury was revived, and at the same time a considerable quantity of water was produced. It has even run down in drops in the inside of a vessel which contained about five ounce measures of the air. The air which remained after this process was about half as much as the original quantity of alkaline air, and it had in it a quantity of undecomposed dephlogisticated air. For with two equal measures of nitrous air, the standard of it was 1.4, and there was nothing inflammable in it.

Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three measures of it were reduced to two. Water was produced in the process, and the air that remained was considerably dephlogisticated; the standard of it, with a mixture of two equal measures of nitrous air, being 1.7.

But

But in another experiment of this kind, there must have been a quantity of inflammable air set loose from the alkaline air, as well as dephlogistified air from the red precipitate; because, after the operation had continued some time, there was a violent explosion within the vessel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I possibly could, in order to satisfy myself with respect to the production of the *water* which I had found before; and there was time enough before the explosion to observe that water was certainly produced in the process. To appearance the quantity of air was never much diminished.

That water should be produced when the scales of iron are heated in alkaline air, which I found to be the case, is not extraordinary, as the same is found on heating this substance in inflammable air; and it appears to contain within itself the water that is expelled from it.

In this case it must have come, in part from the alkaline air, which I have observed, certainly contains it, and in part from the precipitate; and since it is capable of yielding dephlogistified air by heat, it must contain water, which enters in a great proportion into this kind of air.

B O O K VII.

MISCELLANEOUS EXPERIMENTS AND OBSERVATIONS RELATING TO AIR.

SECTION I.

Of the Aerial Form of Substances.

THOUGH, since the accidental discovery of the *marine acid air*, I have purposely sought for others, and not without success, having found several other kinds of acid air, besides the *alkaline principle*, which I have also been able to exhibit in the same form, free from any combination with water, I by no means conclude that I have discovered *all* the kinds of air that may exist in nature; or, in other words, all the kinds of substances, simple, or compound, that are capable of being reduced

duced to a dry and permanently elastic vapour. For I believe there is no substance in nature but what is capable of assuming that form, in a *certain degree of heat*. And though my experience in these matters might enable me to judge pretty well *a priori* what substances are likely to yield air, and of what kind it will be, I have frequently been exceedingly disappointed in my expectations in this respect; and I have reserved for this place the mention of several substances, and combinations of substances, which, in general, contrary to my expectations, though not always so, yield no air that I can discover, and I shall be glad to be informed by any person *why* they do not.

1. Having expelled a permanently elastic fluid, to which I give the name of *air*, from spirit of salt, volatile spirit of sal ammoniac, and other fluids, I took it for granted, that from *radical vinegar*, which is the pure *vegetable acid*, in its state of greatest concentration, I should have got a true vegetable acid air; but it certainly yields no such thing by the application of heat only. Though radical vinegar yields air by the dissolution of some metallic substances, I could not make it give any air by the help of any other substances containing phlogiston, which is the case with oil of vitriol; and I gave it, for this purpose, a boiling heat, with all the substances following, viz. *copper, quicksilver, bis-*

nut, charcoal, oil of turpentine, spirit of wine, and liver of sulphur, which last, however, yielded a little fixed air; but this must have been contained in it before.

2. I was hardly ever more surprized than when I found that I could get little or no air from the *smoking liquor of Labavius*, though the fume issuing from it is so exceedingly dense. I heated a considerable quantity of this liquor prepared by Mr. Woulfe, and which he obligingly brought me for the sake of the experiment, which he himself suggested, as not likely to fail. This I did in a glass phial with a ground stopper, intending to receive the produce in quicksilver. But little or nothing came over, besides the common air which had lodged in the top of the phial, and which appeared not to have been in the least injured after the experiment. A very small part, however, of what came over, seemed to be absorbed by water; and also when, in the presence of Mr. Woulfe, I endeavoured to convey the air that might be expelled from this liquor into a quantity of alkaline air, a slight degree of *whiteness* was produced. But both these effects might have been occasioned by a little redundant spirit of salt contained in this liquor.

3. I have not yet been able to combine either the fixed or the volatile *alkali*, with any substance
whatever,

whatever, so as to form a permanent air, though the alkaline principle itself may be exhibited in that form. Some facts had been mentioned to me by Mr. Woulfe, which led us both to conclude, that air might be procured from it by means of a solution of *iron*. But when, in consequence of this hint, I endeavoured to procure air from iron by means of the alkalis, the experiments which I made for this purpose did not succeed. I boiled a quantity of very fine iron wire in caustic fixed alkali, after finding that nothing could be produced from it without heat; and I put another quantity of the same wire into a strong volatile spirit of sal ammoniac, and also let some of it remain about a month in alkaline air; but in all these cases, without any visible effect. At the end of the last-mentioned process, water being admitted to the air, absorbed the whole of it instantly, which shewed that it was perfectly pure, and had got nothing from the iron. Also, no air was produced from liver of sulphur, dry or moist, in alkaline air; and they continued together almost two days.

4. I expected to have expelled some kind of air (and at that time thought it not impossible but it might be even phlogiston itself in the form of air) from *spirit of wine*. But, like water, it was only converted by heat into *vapour*, which was con-

densed again in the cool recipient. The addition of *camphor* to it produced no other effect.

I was not without hopes, at one time, that the matter of the *odoriferous particles*, which affect the sense of smell, might have been exhibited in the form of permanent air, as well as the exhalations of acid or alkaline substances; but though I made the experiment with several odoriferous substances, some grateful, and others offensive to the nostrils, it has hitherto been without any effect.

I most sincerely wish that the *reason* of these phenomena may be fully investigated; as such an investigation, if successful, would probably carry us a good way into the knowledge of the ultimate constitution of natural bodies, and help us to explain some of the most fundamental laws respecting them. Hitherto I do not find that this subject has even been thought of by philosophers.

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SECTION II.

*Experiments relating to the seeming Conversion of
Water into Air*.*

SINCE many persons have expressed a wish to be acquainted with the experiments I have lately made, which at first seemed to favour the idea of a *conversion of water into air*, but which terminated in the discovery of a fact, in my opinion, still more extraordinary, I shall submit to the Royal Society the result of the observations I have already made; though, as yet, I have by no means been able to satisfy myself so fully as I could wish with respect to some particulars connected with the subject. All the *facts* which I shall state may be depended upon; but it is probable, that different persons may draw different *conclusions* from them; and to mere *opinions*, I have never shewed myself much attached.

* This section is taken from the Philosophical Transactions, vol. lxxiii. p. 414.

Having formerly observed several remarkable changes in fluid substances, in consequence of long exposure to heat in glass vessels hermetically sealed, I then formed a design of exposing all kinds of solid substances to great heats, in a similar state of confinement; and for that purpose provided myself with a cast iron vessel, which I could close at one end, like a digester, and of such a length, that one of the ends might be red hot, while the other was sufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off steam, or air, in any period of the process.

I imagined, that when substances, consisting of parts so volatile as to fly off before they had attained any considerable degree of heat, in the usual pressure of the atmosphere, were compelled to bear great heats under a greater pressure, they might assume new forms, and undergo remarkable changes, similar to what we may suppose to be the case within the bowels of the earth, where, by means of subterraneous fires, various substances bear great heats under very great pressures.

I have had this instrument some years; but it was so ill constructed, that I could not make the use of it that I had originally intended. I therefore lately fitted up some gun barrels in the same manner, and made my first experiment with lime stone;

stone; expecting that when the fixed air, and other volatile matters, that might be contained in it, should be compelled to bear a red heat, without a possibility of making their escape, the substance itself might undergo some change; but I had no particular expectation concerning the nature of that change. I had, however, been so often favoured with valuable results, from merely putting things into new situations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight, and steam-tight, under so great a pressure as I wished to apply.

I was mentioning these ideas to Mr. Watt, in whose neighbourhood I have the happiness to be situated, when he mentioned a similar idea of his, viz. that of the possibility of the conversion of water, or steam, into permanent air; saying, that some appearances in the working of his fire engine had led him to expect this. He thought that if steam could be made red hot, so that all its latent heat should be converted into sensible heat, either this or some other change would probably take place in its constitution. The idea was new to me, and led me to attend more particularly to my former projects of a similar nature, and I began with lime stone, wishing to try the effect of giving a red heat to lime, in which water only should be previously combined,
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thinking it might possibly have the same effect with making the water itself red hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I exposed it gradually to a strong heat in an earthen retort, such as I had been usually supplied with by Mr. Wedgwood (who is as much distinguished by his love, and generous encouragement, of science, as he is by his improvements in his own curious art) not imagining that it could make any difference whether the lime, so prepared, should receive its heat in an earthen retort, or in a vessel of iron or glass. Proceeding, however, in this manner, I found that nothing came over in the form of *steam*, but that there was a great quantity of *air*, several hundred times more than the bulk of the water, and at that time there was in it a considerable proportion of fixed air, which I imagined might either be that which had not been sufficiently expelled before, or might be composed of some phlogistic matter contained in the lime, and the purer air that was yielded by the water. For I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was such as a candle would just burn in) came from the water, especially as in some of the processes, the weight of the air I caught was very nearly, if not quite equal to that of the water,

water, and interposing a large glass balloon between the retort and the recipient for the air, I observed that it remained perfectly cool and dry during the whole process; and several hours afterwards there was not the least moisture condensed in it. I also received a quantity of another produce of air, made in this manner in mercury, and having viewed it with the greatest attention, observed that, after several days, it never deposited the least moisture.

I then calcined a quantity of natural lime stone, with this glass balloon interposed in the same manner, and found no water, but only air to come from it, though the stone is generally supposed to contain water. But when I used much more than half an ounce of water to the quantity of whiting or lime above mentioned, I always had some water come over, though very little in proportion to the quantity made use of.

I did not fail to examine whether there had been any loss in the weight of the lime, or whiting, in order to determine whether any part of these solid substances had entered into the composition of the air; but I found much difficulty in weighing them with exactness, after shaking them out of an earthen retort, into which I could not see, and to which part of these earthy matters often adhered, so that I could not obtain much satisfaction even when I
broke

broke the retort. Besides, there was always some loss of the earth in the cloudiness of the air, whenever the production of it was rapid. In a future process I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was necessary to the conversion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined that by this means the matter, or principle, of *heat* was so intimately combined with it, as not to be separated from it by cooling, as in the case of steam. But I, as well as all my friends, was a long time utterly disconcerted upon finding that when I put the whiting and water into a coated glass retort, the water came over in the form of steam, and little or no air was produced. The result was also the same when I made the process in a gun barrel, in a porcelain retort, or even in an earthen retort glazed in the inside.

That the earth had not lost its property of doing its part in the business, I found by putting more water to the same whiting which had failed in the glass retort, and which had been used no less than four times before, and then heating it in an earthen retort; when again it gave air only, and no water,

the same as before. And at this time I observed, that part of the air was hardly to be distinguished from that of the atmosphere.

I cannot express my surprise at my unexpected failure with the glass retort; and my speculations on the subject were various, but at that time altogether ineffectual. Among other things it occurred to me that, possibly, some phlogiston, either contained in the earthen retort, or coming through it (though I could not tell *how*, or on what principle) from the fire, might be necessary to water, and all other substances assuming the form of air. But when, with this idea, I put spirit of wine, oil, or iron filings to the lime, I got nothing from these mixtures in glass retorts besides steam and inflammable air, from the decomposition of these substances containing phlogiston.

That there was nothing in the materials of which the earthen retort was made, that necessarily produced the air, was evident from my not succeeding when I pounded a broken retort, and heated it, mixed with water, in one of glass.

Being satisfied that the production of air depended very much upon the retort itself, I thought of using the retort only with water, but without any lime, or earthy substance; and I found it succeed far beyond my expectation. For when I put a small quantity of water into one of these retorts, and endeavoured

deavoured to distil it gently, I never failed to procure about an hundred ounce measures of air ; and this I could do as often as I pleased, with the same retort, and without its losing any weight ; and the air produced in this manner never had any portion of fixed air in it, and was always but very little inferior to that of the atmosphere.

In all these processes I observed, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I exposed the retort to the fire quite full of water, or with only about an ounce measure of water in it, or even after letting it remain full for a short time, and then pouring out all that I could from it ; so that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in some measure united to the substance of it, that had contributed to this production of air.

These retorts (which Mr. Wedgwood informs me are made of a mixture of fresh and of burnt Devonshire pipe clay) are pervious to water, though not to air ; so that while the air is produced from that water which has entered the pores, the rest is sometimes visibly making its escape in the form of a copious smoke on the outside. It was evidently impossible, however, as I then thought, and contrary to all the laws of hydrostatics, that air should enter by

the same pores by which the water or steam was escaping, and at the same time that its endeavour to force its way out of the retort was such that it overcame a considerable resistance from the column of water, at the mouth of my recipients. Air might have *escaped* through any unobserved pores in the retort, but none could have *entered* that way; and if there was the least sensible crack in any part of the retort, I was never able to collect any air at all*.

But the following experiments shewed, as I thought, that it is sufficient for the production of air that steam come into contact with clay sufficiently heated. Between a copper still and the glass tube communicating with my recipient for air, I introduced the stem of a tobacco pipe; and by means of a small furnace, I kept about three inches of the middle part of it moderately red hot. In this state, making the water boil, I uniformly received air, though mixed with steam, at the rate of five ounce measures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but steam was delivered by it without any air at all. There was no fixed air in this produce, and it was all such as a candle would hardly have burned in it. It might, I thought, have been better, and also more in quan-

* It will appear, however, that the air must have entered by the pores, but by means of a power very different from that of *pressure*, and able to counteract it.

tity, if I had not used the stem of a foul pipe. But when I used a clean pipe in the same manner, I did not find the air much, if at all, improved. Suspecting this to arise from the near contact of the fuel, I inclosed the tobacco pipe in an earthen tube, and then I had air as good as I had generally got in the earthen retort, and not much worse than that of the atmosphere.

Another circumstance I observed was, that if the outside of the vessel which contained the water or steam, through which it passed, when the requisite heat was applied to it, was not dry, or perhaps surrounded with good air (for in those circumstances the following experiment differs from the preceding ones) the experiment did not succeed.

When I put the ball of an earthen retort, filled with moist clay, into an iron digester, and applied heat to it, I got only a very little fixed air (which was probably composed of a small quantity of air beginning to be produced from the materials) and inflammable air from the vessel. All that came over besides was steam, and at last inflammable air from the vessel itself.

Being now able to procure air by means of water, in this most simple method, *viz.* by water only in the earthen retort, I had an opportunity of ascertaining, with great ease and exactness, several circumstances relating to the process, and of obviating,

as I thought, some objections to the conclusion that I had drawn from it. Among other things, I fully satisfied myself that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having used the same retort till I had got from it nearly an ounce weight of air, or 800 ounce measures, I found that it had not lost so much as a single grain in weight. After the first process it weighed just three grains more than it did at first, and it continued to weigh the same till after the last process. This small addition of weight might easily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the processes were over, I kept the whole retort in a red heat for several hours, and then found that, besides losing those three grains, it weighed eight grains less than it did at first.

Before this I had found, that the calcined whiting, which I had used in the first experiment, could not, as some had supposed, attract from the atmosphere, any considerable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity that I generally made use of) did not attract more than eight grains of any thing when it was exposed a whole day in an open dish, though it had lost more than half its weight in calcination.

It had been imagined by some, that the air which I got in these earthen retorts was that which had been attracted from the atmosphere by the inside surface of them. But, besides that no air could ever be produced without water, to obviate this objection more particularly, when one of these retorts was giving its last portion of air, I immersed the mouth of it in a basin of water; and letting it cool in that situation, filled it again without admitting any access of air to the inside; and yet, on repeating the process with it, the air was produced just as freely as before. This operation I repeated several times. If it be said, that the outside of the retort attracted the air, still the inside, being composed of the same materials, must have attracted air also; and it would have appeared by the ascent of the water from the basin, the retort being sufficiently impervious to air.

By some it was imagined, that either the air itself that I procured, or at least the power of the retort to contribute to the production of it, was owing to something that was transmitted from the burning coals, but which could not pass through glass or metals. To determine this, I took an earthen tube, of the same composition with the retort, and putting a little water in it, placed it, surrounded with sand, in a glass vessel, and this again, surrounded also with sand, in an iron one; and yet the heat transmitted
1 through

through all these substances, enabled the earthen tube to give air, in the same proportion, and of the same quality, as it would have done if it had been exposed to the naked fire.

Having now procured air, by means of water, in a very simple, and as I thought, an unexceptionable manner, I wished to make it in greater quantities, in proportion to the water employed; and for this purpose I first thought of increasing the size, or the thickness of the porous retorts; but I thought it might answer as well if I put into the retort, in powder, the materials of which they were made, or other substances of the same kind.

Accordingly, by mixing ground flint and clay in various proportions, I presently increased the quantity of air much beyond my expectation. In the first trials, in which I had much flint and a little clay, I never failed to get 200 ounce measures of air from one of water. Then, using more clay and less flint, I had still more air; and at last, leaving out the flint altogether, and using clay only, I never failed to get much more than 400, and generally between 500 and 600 ounce measures of air from one of water, which was about three fourths of the weight of the water; and in one particular process I procured very little less than nine tenths of the weight of the water in air, and this air was never much less pure than that of the atmosphere. Some-

times it could not be distinguished from it at all by the test of nitrous air ; and once or twice I thought it even purer than that of the atmosphere.

I must here observe, that I found it not convenient to put so much water to any quantity of clay as would make it cohere in one mass, but only so much as that it should remain in the form of powder. By this means it might easily be poured out of the retort when the experiment was over.

The weight of the water expended in this production of air I ascertained, in the most unexceptionable manner, by weighing the retort, with all its contents, before and after the process. I shall explain this by the result of two of the processes. In one of them, the retort and moistened clay together, lost in weight 588 grains, after yielding 741 ounce measures of air, which (in the proportion of six grains to one ounce measure) would have weighed 444 grains, and consequently, about three fourths of the weight of the water.

In the other process, the loss of weight was 358 grains, after yielding 556 ounce measures of air, which would have weighed 333 grains. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I also found now, that so much heat as I had hitherto applied was neither necessary nor useful.

ful. In the last mentioned process the retort was constantly suspended about six inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a Fahrenheit's thermometer did not shew more than 210° . With this moderate heat I got 465 ounce measures in the course of about twelve hours. When the retort was suspended within six inches of the fire, the air was generally produced at the rate of thirty ounce measures in five minutes. But a thermometer, the bulb of which was immersed in the clay, was still only at the heat of boiling water.

In all these processes, however, there was evidently some loss of water; for, excepting the first experiment with the lime, I never got the whole weight of the water in air; and it might be said that I only expelled the air before contained in the water, though from these experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that escaped through the pores of the retort in the following manner.

Having put the moistened clay into an earthen tube, to which I had fitted a cock and a long glass tube (by means of which I could collect all the air that came from it) I put this within an iron tube, which was closed at the end next the fire, but open at the other end, and so long that I could easily

keep this open, and quite cool, while the other was in the fire; consequently, whatever water escaped through the pores of the earthen tube, it would be condensed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, estimated by the loss of weight in the earthen tube and its contents. I also found, that the water so collected served for the production of more air, just as well as any other water whatever, so that there had been no decomposition of the water in the case.

In the last process that I went through of this kind, the loss of weight in the earthen tube, or rather of the water contained in it, was 392 grains; the air collected was 173 ounce measures, which would have weighed eighty nine grains, and the water which escaped through the pores of the earthen tube, and which I collected, was nearly 195 grains; so that the air and this water together weighed 302 grains, or ten grains more than the original water. But as I estimated the weight of the water only by the space which it occupied in a cylindrical glass tube, divided according to ounces and parts of ounces of water, it was not easy to avoid an error of a few grains. At other times there was an error of a small magnitude on the other side. But it will appear

appear hereafter, that more steam must have escaped invisibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the same time that the water was making its escape out of them, I thought I ascertained pretty satisfactorily by immersing the bulb of it in mercury, contained in an iron vessel. In these circumstances I obtained air as usual, only the produce was not so rapid. In this way, however, I procured above an hundred ounce measures of air from moistened clay; and I discontinued the process without perceiving any termination of it. But the moment the retort was raised out of the mercury, it gave air three times as fast as it had done before. The quality of the air was the same in both cases, *viz.* a little worse than that of the atmosphere.

I even collected thirty ounce measures of air when the bulb of the same retort was immersed in hot linseed oil; but the production of air gradually ceased, and the next day I found the retort almost full of the oil, which had soaked through it. Distilling this oil, I got 300 ounce measures of air wholly inflammable, except a very few ounce measures at the last, which were only phlogistified.

Another presumption in favour of the generation of our atmosphere from water was, that the purity of

the air that I produced from it is so very nearly the same with that of the atmosphere. And the degree of heat requisite to produce it is no greater than may be given by the rays of the sun in certain circumstances. Subterraneous fires, however, would be abundantly sufficient for the purpose, as it appeared to be sufficient for the conversion of water into respirable air, that it come into contact with clay, and perhaps many other earthy substances in the form of vapour. I must, however, observe, that when I threw the focus of a burning lens upon a quantity of moist clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay exposed in an open dish, and also confined in a short earthen tube. Had I then proceeded to repeat this last process with a communication between the inside of the earthen tube and the external air (as I then proposed to do, but was prevented) I should much sooner have discovered what I did afterwards, *viz.* that there was no real conversion of water into air in this process.

The difficulty that strikes many persons most forcibly, is the want of analogy between the conversion of water into air with any other known facts in philosophy, or in nature. But admitting that this conversion is effected by the intimate union of what is called the *principle of heat* with the water, it appears

to

to me to be sufficiently analogous to other changes, or rather combinations, of substances. Is not the acid of nitre, and also that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonstrable, that the acid of nitre is convertible into the purest respirable air, and probably by the union of the same principle of heat.

By the same process by which respirable air seems to be made from water, inflammable air may certainly be made from liquid substances containing phlogiston. Making spirit of wine to boil in a glass retort, I made the vapour pass through the stem of a hot tobacco pipe, and found that it was all converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool, no air was produced, but only vapour, which was instantly condensed in the water.

Being now master of a new and easy process, I was willing to extend it to other liquid substances; and I presently found, as I then imagined, that, by this means, I could give a permanent aerial form to any liquid substance that had been previously thrown into the form of vapour.

When I made the vapour of spirit of nitre, heated in a glass retort, pass through the stem of the hot tobacco pipe, I got as pure dephlogisticated air

as

as ever I have procured from nitre; though the cork, by which the retort was connected with the pipe, was dissolved, and must have contributed to contaminate it, and give it a slight mixture of fixed air.

With oil of vitriol I got air considerably phlogisticated, so that a candle would not have burned in it; but this I also attribute to the cork, which was dissolved in the process. The result was nearly the same when I used water impregnated with vitriolic acid air, though the cork was not dissolved. But this acid is known to contain much phlogiston.

When I used water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the residuum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the same quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air considerably phlogisticated; though some of it was so pure that a candle would have burned in it.

N. B. In all these experiments with the tobacco pipe, all the air was remarkably turbid, like milk,
and

and even the common air in the retort before the process properly began.

In this state of the experiments I think I may venture to say, that no person could have seen them without concluding that there was a real conversion of water into air, there being no known principle, or fact, in philosophy, that could have led any person to suspect a fallacy in the case. In this, therefore, I must have acquiesced, as indeed did all my acquaintance, even those who had been the most incredulous on the subject, after they had themselves seen the experiments. But I was led to the farther prosecution of this business, in consequence of having observed that the *purity* of the air which I procured depended upon the state of that which was immediately contiguous to the earthen retort, or tube, in which I supposed the conversion to have been made; and that some communication with the atmosphere was necessary to the production of any air, as in the experiment with the digester, and those with the clay and the burning lens. And since pure external air was necessary in order to procure good air, it was concluded by several of my friends, and especially Mr. Watt, that the operation of the earthen retort was, to transmute phlogiston from the water contained in the clay to the external air; and that the water, thus dephlogisticated, was capable of being converted

verted into respirable air by the intimate union of the principle of heat.

In order to ascertain what the influence of the external air in this case really was, I inclosed an earthen retort filled with moistened clay, in a large glass receiver, open at both ends, through the upper orifice of which (being narrow) I thrust the neck of the retort, luting it so as to be perfectly air tight; and placing the receiver in a basin of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glass tube, through which I could receive whatever was produced in the process*. In this situation I heated the retort, by means of Mr. Parker's excellent burning lens, when air was received through the tube communicating with the inside of the retort as usual; but at the same time the water rose within the receiver. This effect might be owing to a phlogistication of the air within the receiver; but it was soon diminished far beyond the utmost limit of that process, so that very little of it remained; and examining this air, I found it to be nearly the same with that of the atmosphere, as also was that which came from the retort.

* Pl. VII. fig. 1. exhibits a view of the disposition of the apparatus in this experiment, and in many others of the same nature.

This experiment made it probable, that the air on the outside of the receiver had actually passed through it; and yet it was contrary to all the known principles of hydrostatics, and even any thing hitherto known in chemistry, that air should be transmitted through a vessel of this kind, and in a direction contrary to that in which it would have been forced by the pressure of the atmosphere; while the water, with which the clay was moistened, went the other way. For had the retort been pervious to air, as the inside had a free communication with the atmosphere, the water could not have risen within the receiver. This, however, appeared to be the case, by the following decisive experiments.

Having filled the earthen retort with the moistened clay as before, I made the inside of the receiver perfectly dry, and placed it in a basin of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inside of the receiver, and remained upon the mercury, which rose within the receiver, while air was received from the retort as usual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or something that had the smell of it, was transmitted

mitted from the clay through the retort in the same manner.

I then filled the receiver with inflammable air, and upon heating the retort, it was all drawn through it, and delivered as strongly inflammable as ever by the tube communicating with the inside of it; while the water rose within the receiver, and even covered the retort, which was fixed at the very top of it, so that hardly any of the inflammable air remained within it. In like manner nitrous air passed through the retort unchanged.

From these experiments it seemed impossible not to infer, that the clay of the earthen retort, being thus heated, destroys for a time the aërial form of whatever air is exposed to the outside of it; which aërial form it recovers after it has been transmitted in combination from one part of the clay to another, till it has reached the inside of the retort, while the water is drawn through it in a contrary direction*.

Had this hypothesis been proposed *a priori*, it would, I doubt not, have been thought more extraordinary than the conversion of water into air.

* Notwithstanding this, reasons will presently be given, which shew that this is no just conclusion, and that the process must have been carried on by means of pores.

The great difficulty with respect to the experiment with the lens is, that the water should pass through the retort one way, and the air the other, and yet that the air should not be able to pass without the water. It is also not a little extraordinary, that the weight of the air and that of the water should be so nearly equal.

I cannot conclude this account without acknowledging my obligation to Mr. Parker for the use of his incomparable lens, and his obligingly assisting me in the management of it. Indeed, without this very instrument, or one of greater power than my own, I do not know that the last-mentioned experiments could have been made at all; certainly not to so much satisfaction*.

When I wrote the preceding part of this section, it was my opinion, that the air which was transmitted through the earthen retort, had lost for a moment its *form of air*, and was properly incorporated with the *earth*, as fixed air is in lime stone, and that it had resumed its aërial form in the inside of the retort; because these vessels had been air tight in all my other experiments. But though these earthen retorts be sufficiently air tight for many purposes, and we cannot blow through them

* By means of small earthen vessels, of an inch diameter in the bulb, such as is represented in Pl. VII. fig. 2. the experiment is made with sufficient advantage, with a lens of twelve inches in diameter.

when

when they are plunged in water, yet unless they be *glazed*, they are not so air tight, but that, with an air pump, air may be drawn through them. Consequently, any power acting with the force of an air pump (which is only equal to the pressure of the atmosphere) may overcome the difficulty of air passing through the small pores of such retorts; and there are many powers in chemistry much greater than this.

At present it is my opinion, that the agent in this case is that principle which we call *attraction of cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case, I am far from being able to explain. Much less can I imagine how *air* should pass one way, and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other. I am satisfied, however, that it is by means such pores as air may be forced through, that this curious process is performed, because the experiment never succeeds but in such vessels as, by the air pump at least, appear to be *porous*, though in all such. But the smallness of the pores seems to be rather an advantage in this experiment; and the smallest capillary tubes also are known to act with the greatest force.

A vessel scooped out of *chalk* answered as well as an earthen retort; so also did one of *free-stone*,
first-

first about a quarter of an inch in thickness, and then very near an inch, which I got made for the purpose; and so also did a vessel of *white marble* of the closest texture, though it is sufficiently air tight for most purposes, and nothing but the air pump could have convinced me that it had any pores.

In order to measure the force of this attraction, I placed the glass vessel through which I transmitted the rays of the sun (and which contained the air that was to be drawn through the retort) in a basin of mercury, and found that while the vapour of water was transmitted from moistened clay in the inside of the retort, into the inside of the glass vessel (and consequently the air passed the contrary way, so as to enable the atmosphere to press the mercury into the inside of the glass vessel) it rose three inches and a half above the level on the outside of the glass. It might probably have risen something higher, if all the water in the clay had not then been expended. This was a vessel made on purpose, of pipe clay only, and very compact. When I used more porous earthen vessels, I was never able to raise the mercury so high. For when the pores are large, the mechanical action occasioned by the weight of such a column of mercury counteracts the chemical one, and prevents the transmission of the air or steam.

Having sufficiently ascertained what kind of *vessels* would answer for this experiment, I then tried what kind of *fluid substances* would, by their transmission in one direction, promote the passing of the air in the other; and I found that *oils*, as well as *spirit of wine*, answered this purpose perfectly well; and so also did *mercury*, but not any other metal made fluid by heat.

The mercury passed through the retort in the form of a subtle *black powder*; and on this occasion I observed that the vessel of pipe clay with which the experiment was made, was so far air tight, that when I took it to the air pump, nearly full of mercury, quite hot from the experiment in the sun, and placed it upon the plate of the air pump, I was not able to force any mercury through it. Probably this black powder was much more attenuated than mercury in its natural fluid state can be. There is also a degree of repulsion between white mercury and all glass or earthen vessels. This repulsion mechanical pressure might not be able to overcome, though this chemical action could do it. It remains, however, to be considered, how the mercury in this process could be changed from its natural fluid state into this black powder.

While I was making these experiments with mercury, I took the opportunity of trying one of the

the kinds of air that cannot be confined by water; and having filled the glass vessel with *alkaline* air, and having mercury in the vessel of clay, I soon found, by the smell, that the alkaline air was passing one way, while the mercury went the other, just as readily, I doubt not, as inflammable or nitrous air had gone one way, while water had gone the other.

Of the *metals* in a state of fluidity, I tried *lead*, *tin*, *zinc*, and *bismuth*; melting them in porous earthen retorts; but neither did any metal pass one way, or air the other.

I made some attempts to perform this experiment with vessels which had *visible pores*, especially with fine capillary tubes of glass; having the stoppers of the glass tubes made with fine perforations of this kind, but hitherto without success; and yet I do not absolutely despair of succeeding in an experiment of this kind, and thereby seeing a little farther into this curious process.

SECTION III.

Experiments on Ether.

IT is something extraordinary that, though ether, as I found, cannot be made to assume the form of air (the vapour arising from it by heat, being soon condensed by cold, even in quicksilver) yet that a very small quantity of ether put to any kind of air, except the acid, and alkaline, which it imbibes, almost instantly doubles the apparent quantity of it; but upon passing this air through water, it is presently reduced to its original quantity again, with little or no change of quality.

I put about the quantity of half a nut shell full of ether, inclosed in a glass tube, through a body of quicksilver, into an ounce measure of common air, confined by quicksilver; upon which it presently began to expand, till it occupied the space of two ounce measures. It then gradually contracted about one sixth of an ounce measure. Putting more ether to it, it again expanded to two ounce measures; but no more addition of ether would make it expand any farther. Withdrawing the quicksilver, and admitting water to this air, without

without any agitation, it began to be absorbed; but only about half an ounce measure had disappeared after it had stood an hour in the water. But by once passing it through water the air was reduced to its original dimensions. Being tried by a mixture of nitrous air, it appeared not to be so good as fresh air, though the injury it had received was not considerable.

All the phenomena of dilatation and contraction were nearly the same, when, instead of common air, I used nitrous air, fixed air, inflammable air, or any species of phlogisticated common air. The quantity of each of these kinds of air was nearly doubled while they were kept in quicksilver, but fixed air was not so much increased as the rest, and phlogisticated air less. But after passing through the water, they appeared not to have been sensibly changed by the process.

These experiments were made with vitriolic ether. Having, since, procured a quantity of *nitrous ether*, made by Mr. Godfrey, I had the curiosity to try whether this would produce the same effect; but I found that it increased common air only about one sixth of its bulk. After this mixture had continued two days and a night, water absorbed the ether, and left the common air exactly, or very nearly, the same as before, judging by the test of nitrous air.

SECTION IV.

*Of the Difference of the Quantity of Air by the rapid
Production of it.*

I Have more than once found, that when I expelled air from several substances by heat, the produce was different in quantity, according as the heat was *slowly* or *suddenly applied*; though, at the last, it was always made the same, and was continued as long as any air could be produced. The first observations of the kind were those in which *inflammable air* was produced from vegetable and animal substances. I afterwards made another experiment of the kind in the production of *dephlogisticated air* from red lead and spirit of nitre; using every precaution that I could think of to prevent my imposing upon myself, with respect to a circumstance which, I own, appears rather extraordinary. I imagine, however, that when the heat is applied slowly, some parts of the substances, that are less disposed to volatilize, become, by degrees, *accustomed*, as it were, to bear a force, which, if it had been applied suddenly, would have inevitably
broke

broke their cohesion with the rest; which is in some measure similar to what is observed with respect to the *load stone*, which will sustain more weight when it is applied gradually, than it can be made to do when it is applied all at once.

From equal quantities of the same red lead, without any mixture of spirit of nitre, and using the same gun barrel, I got, by means of heat suddenly applied, more air than I got by heat applied slowly in proportion of *ten* to *six*. The proportion of fixed air was the same in both the cases, and the remainder equally dephlogisticated.

Moistening a quantity of red lead very well with strong spirit of nitre, and putting equal quantities of it at different times into the same gun barrel, in the first process I applied the heat pretty suddenly, and in the second very slowly; and in the first I got three times as much air as in the other. The air was also equally pure in both these cases, and contained equal proportions of fixed air; but towards the end of the slower process the air became nitrous.

From an ounce of red lead, by a sudden and brisk heat I got above two quarts of air, a great part of which was fixed air, and the rest was about twice as good as common air; and immediately after, putting the very same quantity of the same parcel of red lead into the same gun barrel, by heat

very *slowly applied*, but urged vehemently at last, I got no more than two ounce measures of air, a great part of which was fixed air, and the rest not so good as common air.

From an ounce and a half of whiting, in a gun barrel, I got, with a sudden and great heat, 240 ounce measures of air, of which some portions contained nine tenths of fixed air, and the residuum burned with a lambent blue flame. Repeating the same process very slowly, I got no more than 150 ounce measures of air from the same quantity of whiting. And again repeating the process with a greater and more sudden heat than at first, I got, from the same quantity of materials, 270 ounce measures of air.

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SECTION V.

*Experiments on the Mixture of different Kinds of Air,
that have no mutual Action.*

CONSIDERING the very different specific gravities, and other remarkably different properties of different kinds of air, it might naturally enough be taken for granted, that those which differ very much in *specific gravity* at least, would separate from each other after they were mixed, the heavier occupying the lower place, and the lighter the upper; and that, by this means, the heavier kind of air might be made to expel the lighter, if there should be an opportunity for its escape from the upper part of the vessel. As different kinds of air will often be unavoidably mixed in a variety of experiments, I thought it a matter of some consequence to ascertain precisely how the fact was in this respect; that I might not, upon any occasion, deceive myself, by imagining that I had one kind of air only, when, in reality, I might have a much greater proportion of some other kind than I suspected.

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The result of my trials has been this general conclusion ; that when two kinds of air have been mixed, it is not possible to separate them again, by any method of *decanting*, or pouring them off, though the greatest possible care be taken in doing it. They may not properly *incorporate*, so as to form a *third species of air*, possessed of new properties ; but they will remain equally diffused through the mass of each other ; and whether it be the upper or the lower part of the air that is taken out of the vessel, without disturbing the rest, it will contain an equal mixture of them both.

After some experiments with common phials, but which I rejected, from the consideration that the agitation of the air within the phial, by the letting in of the water, would necessarily mix the two kinds of air contained in it, though they had been ever so carefully separated, I avoided that inconvenience, by making the experiments in the following more accurate manner, applying it to all the kinds of air that will bear to be confined by water.

Into a cylindrical vessel, which I could open at both ends, having ground stoppers fitted to them, I put equal quantities of *fixed* and *common air* ; and after suffering them to continue a whole day, the vessel remaining in a perpendicular position, in which they had opportunity, and time enough, to have separated from each other, I very carefully
let

let out the contents of the upper part of the vessel only, the water entering slowly at the lower opening; and put the remainder, which had occupied the lower part of the vessel, into a separate phial; then examining them one after the other, I found no sensible difference between them, equal proportions of both being absorbed by water. If there was any difference, it did not, however, exceed the trifling one in the next experiment, made with an eight ounce phial, in the bottom of which I had made a hole, by which the water might enter as the air was let out at the mouth of it.

This phial I filled with equal quantities of fixed and common air; and after a considerable time, I let off the upper part, while the water entered below; and observed that, of the four measures which had occupied the upper part of the phial, two and an half remained unabsoꝛbed by water, and of the lower part, two and a quarter; so that there was a little more fixed air in the lower than in the upper part of the vessel.

Having mixed equal quantities of *inflammable* and *nitrous air* in the phial which had a perforation at the bottom, and letting it out in five different parts, I observed that they all burned with a lambent flame, without any sensible difference between them.

I tried

I tried in the same manner a mixture of *nitrous* and *common air*, and after letting them continue together all night, found that the lower part of this mixture diminished common air a little more than the upper part; but the difference was very small.

I put together equal quantities of *nitrous* and *fixed air*, in the perforated phial; and decanting it at several times, with the same care as before, and mixing the several parts with common air, could observe no sensible difference in the diminution produced by any of them.

Fixed air is equally diffused through the whole mass of any quantity of air from putrefying substances with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the same proportion by passing through water. This is also the case with inflammable air, and air in which sulphur has burned.

At the same time that I was trying the purity of fixed air, I had the curiosity to endeavour to ascertain whether that part of it which is not miscible in water, be equally diffused through the whole mass; and, for this purpose, I divided a quantity of about a gallon into three parts, the first consisting of that which was uppermost, and the last of that which was the lowest, contiguous to the water; but all these parts were reduced in about an equal proportion,

tion, by passing through the water, so that the whole mass had been of an uniform composition.

Having filled the strong glass vessel, in which I usually fire them by the electric spark, with the usual mixture of one third dephlogisticated and two thirds inflammable air, I let it remain in a perpendicular position all night, in which there was certainly time enough for them to have separated from each other, if their very different specific gravities would have disposed them to do so. But though the two wires between which the explosion is made were at the very top of the vessel, which in the case of a separation would have been the region of pure inflammable air (which cannot take fire of itself) yet when the spark passed between them, the two kinds of air were exploded as completely as they could have been immediately after they had been mixed.

It is also observable, that the phlogisticated and dephlogisticated air which compose the atmosphere, are of very different natures, though without any known principle of attraction between them, and also of different specific gravities; and yet they are never separated but by the chemical attraction of substances, which unite with the one, and leave the other, as in all those processes which I have termed *phlogistic*.

The

The same is the case with common air and alkaline, or any of the acid airs. For though all these kinds of air differ in specific gravity from common air, yet if they be mixed with common air, and water be admitted to them, the quantity will decrease more or less slowly in proportion to the quantity of common air in the mixture. Whereas, if the alkaline or acid airs had been heavier than the common air (as the latter, at least, manifestly are) and did not mix with it, the water would absorb them as readily as it does when the jar contains no other kind of air; as, on the other hand, if the common air had been the heavier, it would have protected them from the access of the water, which would not, in this case, be able to come at the acid or alkaline air, and therefore could not absorb any part of the quantity. I have noted, however, one exception to this rule respecting alkaline and inflammable air, which did not seem to mix together.

I have since made a mixture of vitriolic acid air and fluor acid air, and find that they continue intermixed throughout. I mixed equal quantities of them in a jar of quicksilver, and observed, that when water was admitted to the whole mass, the crust was formed equably from the bottom to the top of the vessel.

Notwithstanding

Notwithstanding these experiments, I do not say but that if two kinds of air, of very different specific gravities, were put into the same vessel, with very great care, without the least agitation that might mix or blend them together, they might continue separate, as with the same care *wine* and *water* may be made to do; but that, when once they have been mixed, they will continue to be so, like wine and water, after having been shaken together.

From the experiments above recited, it will follow, that when fixed air, or air of any other kind, is conveyed into a vessel containing common air, a quantity equal to the contents of the whole vessel must be thrown in, before half of the common air be expelled; because half of the air that is introduced will always be expelled along with it; that after another equal quantity, still one fourth of the common air will remain; and after another, one eighth, &c.

SECTION VI.

Of the Expansion of different Kinds of Air by Heat.

FROM a very coarse experiment, which I made very early, I concluded that fixed and common air were expanded alike with the same degree of heat. But by the method which I then made use of, it was not possible to discover small differences in this respect. I have since attempted it in another and more accurate manner, and have applied it to *all the kinds of air* with which I am acquainted; and though I do not think that the results can be intirely depended on, on account of the difficulty that I found in making the experiments, and my not having time to repeat them so often as I could have wished, it may not be amiss to give an account of what I *did* observe, though it should only be a means of putting others upon doing the same thing in some better manner, and with more attention.

I took a phial, containing thirteen ounce measures of water, and filled it successively with all the different kinds of air, having been previously made
very

very dry, and filled with quicksilver. Then holding the mouth of it under the quicksilver, I inserted into it a perforated cork, to which was annexed a long glass tube, as is represented, Pl. V. fig. 5, leaving a little quicksilver in the neck of the phial, that the expansion of the air might drive it along the tube. This phial, so prepared, I placed in a small wooden box, and carrying it into a warm room, carefully marked the place at which the quicksilver stood after it had become perfectly stationary in that degree of heat. Then carrying it into a colder room, and letting it remain there a sufficient time, I marked the place in which it stood in that degree of heat, having a Fahrenheit's thermometer accompanying it in each situation. I then carried it back into the warm room, and observed whether the quicksilver rose to the same place as before; and after using other little precautions, to prevent any mistake, I found the following intervals in the place of the quicksilver in the different kinds of air, the same phial and the same glass tube being used in all the cases. The quantity answering to ten degrees was made by computation, to shew the proportional expansion at one easy view.

	The distance between		inches	10°. being
In Common air	34°	and 48°	was 1.85	1.32
—Inflammable	34	48	2.87	2.05
—Nitrous	33	50	3.44	2.02
—Fixed	32	50	3.97	2.20
—Marine acid	32	50	2.40	1.33
—Dephlogistified	32	51	4.2	2.21
—Phlogistified	32	51	3.15	1.65
—Vitriolic acid	32	48	3.70	2.37
—Fluor acid	46	56	2.83	2.83
—Alkaline	32	50	8.56	4.75

The expansion of the *alkaline* air in this experiment will appear extraordinary; but I must observe that there was a little *moisture* in the phial, which had been expelled from the materials together with air; and to this water, emitting more air by heat, I am inclined to ascribe the much greater dilatation of this than of the other kinds of air; and at that time I could not conveniently repeat the experiment, not having any better materials at hand.

That *inflammable*, *nitrous*, and *phlogistified* air, containing more phlogiston than common air, should expand more with equal degrees of heat, is agreeable to analogy, the same thing being observed
in

in fluids: but how it should happen that *dephlogistated* air, which contains less phlogiston, should expand more than common air, I do not see, and therefore I wish that the experiments might be repeated in some better manner.

SECTION VII.

Of the specific Gravity of different Kinds of Air.

IN order to ascertain the specific gravity of dephlogistated, and other kinds of air, I took a glass tube about nine inches long, and fastening it to the neck of a bladder, which, with such a degree of distension as I could give it, in the manner in which the experiment was made, contained fifty five ounce measures, or one pennyweight nine grains of common air. The tube was so fastened, that I could take it out at pleasure; and having the bladder thus prepared, I carefully compressed it, then filling it in part with that kind of air which I was

about to weigh, I compressed it again, and then filled it intirely; so that I was pretty confident that the air within the bladder contained very little common, or any other kind of air. In this manner I proceeded to weigh *dephlogistified air*, and at the same time *nitrous air*, and *air phlogistified with iron filings and sulphur*, which I take for granted is the same thing with air phlogistified by any other process.

The following short table exhibits the result of all these experiments at one view.

The bladder, filled with	dwts.	gr.
phlogistified air, weighed	7	15
nitrous air	7	16
common air	7	17
dephlogistified air	7	19

This result agrees sufficiently well with my former observations, though they were not made with so much accuracy, viz. that both nitrous air, and air diminished by phlogistic processes, are rather lighter than common air; and it is consonant to this, that, in the present experiment, dephlogistified air appears to be a little heavier than common air:

In

In this experiment, the dephlogisticated air was so pure, that one measure of it, and two of nitrous air, occupied the space of four fifths of a measure. Had the air been more pure, it would, no doubt, have been specifically heavier still.

SECTION VIII.

Of Sound in different Kinds of Air.

ALMOST all the experiments that have hitherto been made relating to *sound*, have been made in common air, of which it is known to be a vibration, though it is likewise known to be capable of being transmitted by other substances. There could be little doubt, however, of the possibility of sound *originating* in any other kind of air, as well as of being *transmitted* by them; but the trial had not been actually made, and I had an easy opportunity of making it.

Besides, the experiments promised to ascertain whether the *intensity* of sound was affected by any

other property of the air in which it was made than the mere *density* of it. For the different kinds of air in which I was able to make the same sound, besides differing in specific gravity, have likewise other remarkable chemical differences, the influence of which with respect to sound would, at the same time, be submitted to examination.

Being provided with a piece of clock work, in which was a bell, and a hammer to strike upon it (which I could cover with a receiver, and which, when it was properly covered up, I could set in motion by the pressure of a brass rod, going through a collar of leathers) I placed it on some soft paper on a transfer. Then taking a receiver, the top of which was closed with a plate of brass, through which the brass rod and collar of leathers was inserted, I placed the whole on the plate of an air pump, and exhausted the receiver of all the air that it contained. Then removing this exhausted receiver, containing the piece of clock work, I filled it with some of those kinds of air that are capable of being confined by water, by means of a bent glass tube inserted into a piece of brass, which I could screw into the bottom of the transfer, so as to introduce the bended tube, through the water of my trough, into a jar containing the air on which I wished to make the experiment.

When

When this was done, I removed the glass tube, and had the receiver filled with that species of air in which I wished to produce the sound, and the apparatus for making the sound within it. Then by forcing down the brass rod through the collar of leathers, I made the hammer strike the bell, which it would do more than a dozen times after each pressure. And the instrument was contrived to do the same thing many times successively, after being once wound up.

Every thing being thus prepared, I had nothing to do, after filling the same receiver with each of the kinds of air in its turn, but receding from the apparatus, while an assistant produced the sound, to observe at what distance I could distinctly hear it. The result of all my observations, as far as I could judge, was that the intensity of sound depends solely upon the *density* of the air in which it is made, and not at all upon any chemical principle in its constitution.

In inflammable air the sound of the bell was hardly to be distinguished from the same in a pretty good vacuum; and this air is ten times rarer than common air.

In fixed air the sound was much louder than in common air, so as to be heard about half as far

again; and this air is in about the same proportion denser than common air.

In dephlogisticated air the sound was also sensibly louder than in common air, and as I thought rather more than in the proportion of its superior density; but of this I cannot pretend to be quite sure.

In all these experiments the common standard was the sound of the same bell in the same receiver, every other circumstance also being the same; the air only been changed, by removing the receiver from the transfer, and blowing through it, &c.

S E C.

SECTION IX.

Of the Power of the different Kinds of Air to conduct Heat.

ONE of the first things that I proposed to try, with respect to the different kinds of air, was to observe their *power of conducting heat*, and I had a contrivance for that purpose when I was at Leeds. The thing, however, appearing to me to be of less consequence than other things that I had in view, I deferred it some time, till, in consequence of the doctrine of heat becoming, by means of Dr. Crawford's book, the subject of general conversation, I determined to execute what I had so long projected.

For this purpose I prepared the vessel represented PL. VI. fig. 2, and described in the *Introduction*. The thermometer was a very sensible one, and the scale large, so that I could mark upon it twenty divisions, each larger than half an inch between the mean temperature of the atmosphere, and a heat much below that of boiling water. After several trials I at length adjusted it in such a manner, that, having

having filled the vessel with any kind of air, I could plunge it to a certain depth, first in *hot*, and then in *cold* water, so that the mercury would rise to the division 20, and fall to that of 6 or 7 in a reasonable time. I had a clock that beat seconds close by me, and was so situated, that I could not well make a mistake of more than two seconds, in noting the time when the mercury came to any particular division. The precautions I used to plunge the vessel the *same depth* in the water, in all the experiments, and to exclude all other differences, except what might be occasioned by the different kinds of air, it would be tedious to recount; and no person conversant in experiments, and who is disposed to repeat them, will need to be so minutely instructed.

I will just observe, however, that in the vessel I used for *hot* water, I always made it boil, and it was so full, that the plunging of my air vessel into it made it run over; and the vessel for cold water was always fresh brought from the same pump. The mouth of the air vessel was in a cup of mercury, always filled to the same height; and by this means I could try in the same manner even those kinds of air, that could not be confined by water.

The best account that I could give of the result of these experiments would be to exhibit them in the

the form of *tables*, of the time at which the mercury reached all the degrees of the scale, both in ascending and descending; which tables I have drawn up. But this I shall defer till I have an opportunity of repeating all the observations. At present I would only observe, that all the differences were not so striking as I expected to have found them, but that *inflammable air* conducted heat much better than any other kind of air, the mercury ascending the same space in about half the time that it took up in common air. *Fixed air*, and all the kinds of *acid air*, conducted heat considerably worse than common air. *Alkaline air* conducted heat rather better than the acid airs, and dephlogisticated air a little worse than common air, but so little, that I would not answer for the same result in repeating the experiment.

N. B. In the course of these experiments, I could not avoid observing so great an expansion of *alkaline air* by heat, that I conclude the observation, p. 450, may be perfectly accurate, though the extraordinary nature of it made me entertain the doubt I have there expressed concerning it.

SECTION X.

Of the refractive Power of different Kinds of Air.

CONSIDERING the very different properties of the different kinds of air with which I have been conversant, it was impossible not to think of the probability of their having different *refractive powers*, and of some method of ascertaining this circumstance. But I am sorry to inform my readers, that my experiments have been without any success.

For this purpose, I procured a prism, consisting of three plates of glass, fastened together by cement, the cavity being large enough to contain about a quarter of a pint. This prism I fixed upon a stand, at the distance of ten feet from a window, in which I had a small apparatus, contrived to throw a beam of the sun's rays into the room. This beam was received by a board, furnished with a piece of brass work, containing several small holes, through any of which I could transmit a beam of light upon the prism, which was placed, in a vertical position, close behind it; and the wall on which
the

the image of the sun was received was twenty feet from the prism.

With this apparatus, which I thought promising enough, I proceeded to try the refractive powers of nitrous and inflammable air; but I could perceive no difference in the place of the image, whether the beam of light was transmitted through the prism, carefully filled with either of these kinds of air, or not through it; allowance being made for a small degree of refraction occasioned by a want of perfect parallelism in the plates of the prism. The result was the very same, whether it contained common air, or either of the two kinds above-mentioned.

Having had so little success with these two very different kinds of air, I thought it would be in vain to try any of the other kinds; and therefore, for the present, have desisted from my pursuit; but I am not without a design to resume it with a different kind of apparatus, if I be so happy as to succeed in the construction of it.

SECTION XI.

Of Air in the Bladders of Fishes.

I Made a beginning of a course of experiments, which, I think, may be pursued to considerable advantage, on the state of the air which is contained in the *bladders of fishes*. It is commonly supposed, that these bladders are of no other use to the fishes than to assist them in rising or sinking in the water: but I have some doubt about this hypothesis; at least they may have some other use. Some fishes, I believe, are not furnished with these bladders. When they are taken out of the fish, the air cannot be got from them by pressure, but I was always obliged to burst or cut them; and yet that the air does change in these bladders, is, I think, pretty evident, from my having found it in different states.

The first time that it occurred to me to examine the air contained in these bladders, I found it, in a great number of them, to be perfectly noxious, not being at all affected by nitrous air. This was on the 31st of May, 1774. But at another time, viz. the

the 30th of March following, I found air that I had pressed from the bladders of the same kind of fishes, viz. roaches, not to be quite noxious, being affected by nitrous air, though not to a great degree. I have not pursued these experiments any farther; but I should think that it might not be difficult, by diversifying them properly to make some discoveries concerning the animal œconomy of fishes, and the use of air to them.

SECTION XII.

Of Changes produced in various Kinds of Air by Exposure to Urine.

THAT phlogiston exhaling from vegetable or animal substances should sensibly affect common air, or dephlogisticated air, which contain little or no phlogiston, and have a strong affinity with it, is far from being extraordinary; but that the same substances which phlogistificate common air, or dephlogisticated air, should likewise affect nitrous air, or inflammable air, which already contain

tain phlogiston (and, as it should seem, to a complete saturation) is a fact that I cannot well explain. This, however, I have observed to be the case with liver of sulphur, iron filings, and sulphur, and various other substances on nitrous air.

Inflammable air I have not observed to have any impression made upon it by these substances, any more than by the electric spark, at least in the temperature of the atmosphere; though, in consequence of simple confinement by water, it has at length, in several instances, lost its inflammability, and, like nitrous air in the preceding circumstances, has become mere phlogisticated air. I have some suspicion, however, that inflammable air may be decomposed by all the same substances that decompose nitrous air, if more *heat*, or more *time*, be given to the process; and perhaps what I considered as *pure water* might, in time at least, have got an impregnation of something that might affect the inflammable air, in those cases in which I found it reduced to the state of phlogisticated air. These suspicions I have been led to in consequence of observing that *urine* had this effect, both on nitrous and inflammable air; an observation which I made accidentally, in the course of exposing a great variety of substances to the sun during the summer of 1779.

Among

Among other things, I had filled a glass tube, about half an inch in diameter, and three feet and a half long, with urine, and had placed it inverted in a basin of the same. In this situation it was kept several months, when it yielded at first a small quantity of air, all of which was afterwards absorbed. After which I perceived crystals to be formed in several places of the inside of the tube, and the urine, from being of a pretty high colour, became very pale.

Seeing no farther change in the urine, and having observed its power of emitting and absorbing air, I exposed to its influence all the kinds of air that could be confined by it, in separate six ounce phials, of which the air of each kind occupied about one fourth, the remainder of the phial containing this old pale urine; and the phials were inverted in basins of the same, and, as it evaporated, were supplied from time to time with more urine.

Things were disposed in this manner on the 27th of July, and I observed that there was no immediate change, either in the inflammable or the phlogisticated air; but the surface of the urine in contact with the common air, dephlogisticated air, and nitrous air, from a pale straw colour, presently became of a deep brown, and especially next the dephlogisticated air. The next morning the colour of the urine in contact with the dephlogisti-

cated air was almost black, and extended through the whole phial. But in the phial in which the common air was confined, the brown colour extended only a little way within the body of the urine. Under the nitrous air the urine was pretty uniformly brown, but not so much so as under the dephlogisticated air. A little both of the dephlogisticated and nitrous air was absorbed, and nearly an equal quantity of each.

The dephlogisticated air having diminished very fast, and becoming thoroughly phlogisticated, I introduced more of the same air into the phial; and I let all the phials stand in this situation till the 22d of July following, when I was obliged to put an end to the process, and I then noted the following appearances. The common air was diminished about one fourth, and was thoroughly phlogisticated; the urine being of an orange colour, but not very deep. The dephlogisticated air, having been renewed, was not completely phlogisticated, but was nearly so. The nitrous air was diminished one half, had changed the urine black, extinguished a candle, and did not affect common air at all, so that it was mere phlogisticated air. But what is remarkable, this phlogisticated air was in a much greater proportion than is generally procured from nitrous air. This effect I ascribe to the *length of time* that the process took up, and there are other

other remarkable facts in confirmation of this opinion.

The inflammable air was diminished to about one eighth of its bulk, and was still slightly inflammable. With more time I doubt not this air would have lost all its inflammability. The urine in this phial was of a very pale colour.

The phlogisticated air alone remained quite unaffected during the whole process, and the urine was of the same colour with that under the common air, viz. a light orange; but this change probably came from that part of the urine, which had been exposed to the common air in the cup, and had gradually extended itself to the urine in the phial.

If the diminution of all these kinds of air was owing to phlogiston, it may be inferred that this principle in the phlogisticated air has a firmer union with its base than it has in nitrous or inflammable air, being less capable of either receiving more, or of parting with what it has got.

But perhaps the most puzzling circumstance in this process is, that the diminution of both the dephlogisticated and nitrous air should be accompanied with the same change of colour in the urine exposed to them. A similar change of colour in a solution of copperas, I thought was owing to the phlogiston deposited from the decomposed nitrous

air. But if this was the cause of the similar change of colour in this case, how came the same change to take place in consequence of the diminution of dephlogisticated air, this diminution being, no doubt, owing to its receiving phlogiston from the urine? It can hardly be that the same change should take place in the colour of the urine, whether it contains more or less phlogiston than it naturally has. Perhaps it may be something common to the constitution both of nitrous and dephlogisticated air, and not phlogiston, that, when they are decomposed, is precipitated, and produces this change of colour in the urine.

SEC.

SECTION XIII.

Of Air in the Calces of Metals.

THAT the calces of metals contain air, of some kind or other, and that this air contributes to the additional weight of the calces, above that of the metals from which they are made, had been observed by Dr. Hales; and Mr. Hartley had informed me, that when red lead is boiled in linseed oil, there is a prodigious discharge of air before they incorporate. I had likewise found, that no weight is either gained or lost by the heating of tin in a close glass vessel; but I purposely deferred making any more experiments on the subject, till we should have some weather in which I could make use of a large burning lens, which I had provided for that and other purposes; but, in the mean time, I was led to the discovery in a different manner.

Having, by some of my early experiments, been led to consider the electric matter as phlogiston, or something containing phlogiston, I was endeavouring to revive the calx of lead with it; when I was surpris'd to perceive a considerable generation of

air. It occurred to me, that possibly this effect might arise from the *heat* communicated to the red lead by the electric sparks, and therefore I immediately filled a small phial with the red lead, and heating it with a candle, I presently expelled from it a quantity of air about four or five times the bulk of the lead, the air being received in a vessel of quicksilver. How much more air it would have yielded, I did not try.

Along with the air, a small quantity of *water* was likewise thrown out; and it immediately occurred to me, that this water and air together must certainly be the cause of the addition of weight in the calx. It still remained to examine what kind of air this was; but admitting water to it, I found that it was imbibed by it, exactly like *fixed air*, which I therefore immediately concluded it must be.

After this, I found that Mr. Lavoisier had more completely discovered the same thing.

SEC-

SECTION XIV.

Of Air supposed to be contained in Mercury.

WHEN quicksilver is boiled in a glass tube, a considerable quantity of air is seemingly discharged from it, and it has been imagined by some that this air had been contained not only in the space between the quicksilver and the glass, but within the pores of the quicksilver itself. If this had been the case, it must have been the property of quicksilver, as of other fluids, to *imbibe air*, and therefore probably to produce a change of quality in the air which it had imbibed, or in that which it left unimbibed. In order to ascertain this, I boiled as much quicksilver as one of the largest of my phials with ground stoppers and tubes would contain, taking care to exclude all the air that I could get from it by the common methods before this operation; and having got a small quantity, I found it to be common air only, applying to it the test of nitrous air. It was probable, therefore, that this air had not been expelled from the body of the quicksilver itself, but only from between the quicksilver and the glass.

After

After this, in order to try whether quicksilver, deprived of all air, had the power of imbibing air, I boiled a pretty large quantity of quicksilver, for near half an hour, and while it was very warm, I put it into a jar, and inverting it in a basin of quicksilver, left a quantity of common air on the surface of it. This air was contracted with cold, but was not sensibly affected in any other manner. For after two days, when I perceived that the diminution had proceeded no farther, I examined it, and found it to be diminished by nitrous air as much as any common air whatever.



END OF THE SECOND VOLUME.

